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[THIRD SERIES.]

ART. IX.—*On the effects produced by mixing White with Colored Light;* by Prof. O. N. ROOD, of Columbia College.

IT was noticed several years ago that when white light was mixed by the method of rotating discs with light of an ultramarine (artificial) hue, the result was not what one would naturally have expected, viz: instead of obtaining a lighter or paler tint of violet-blue the color inclined decidedly toward violet, passing, when much white was added, into a pale violet hue. Two attempts have been made to account for this curious fact: Brücke supposes that the light which we call white is really to a considerable extent red, and that the mixture of this reddish white light with the blue causes it to change to violet. Aubert, on the other hand, following a suggestion of Helmholtz, reaches the conclusion that violet is really only a lighter shade of ultramarine-blue. He starts with the assumption that we obtain our idea of blue mixed with white from the sky, which, according to him, is of a greenish-blue color. We then apply, as he thinks, this idea to the case of a blue which is not greenish, namely, to ultramarine-blue, and are surprised to find that the result is different.

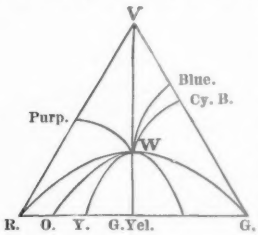
It will be shown in the present paper that these explanations are hardly correct, since they fail to account for the changes, which, according to my experiments, are produced in other colors by an admixture of white. I prepared a set of brilliantly colored circular discs which represented all the principal colors of the spectrum and also purple; these discs were then successively combined in various proportions with a white disc and the effects of rapid rotation noted, a smaller duplicate colored disc uncombined with white being used for comparison.

Under these circumstances it was found that the addition of white produced the changes indicated in the following table:

Vermilion became somewhat purplish.	Cyan-blue became less greenish, more bluish.
Orange became more red.	Cobalt-blue became more of a violet blue.
Yellow became more orange.	Ultramarine (artificial) became more violet.
Greenish yellow was unchanged.	
Yellowish green became more green.	
Green became more blue-green.	

Purple became less red, more violet.

Exactly these same effects can be produced by mixing violet with the above mentioned colors. Let R, G, V represent the three angles of Maxwell's color-triangle, W being the position of white. Now, according to the received theory, as we mix white with different colors we advance in straight lines from the angles or sides of the triangle toward W: in point of fact, however, I find, as a result of the above-mentioned experiments, that we advance in curves toward W, these curves being similar to those roughly indicated in the figure. The only advance in straight lines is along the line joining violet with its complement greenish yellow. The other lines are disposed symmetrically about this line as an axis. These experiments serve to explain the singular circumstance that when complementary colors are produced by the aid of polarized light, it is difficult or impossible to obtain a red which is entirely free from a purplish hue, a quantity of white light being always necessarily mingled with the colored light. In the case of the red, orange, yellow, ultramarine, and purple discs, I succeeded in measuring the amount of violet light which different proportions of the white disc virtually added to the mixture, and found that it is not directly proportional to the amount of white light added, but increased in a slower ratio, which at present has not been accurately determined.



For the explanation of the above mentioned phenomena, Brücke's suggestion that white light contains a certain amount of unneutralized red light is evidently inapplicable, since the effects are such as would be produced by adding a quantity not of red but of *violet* light, and for the present I am not disposed to assume that white light contains an excess of violet light. The explanation offered by Aubert does not undertake to account for the changes produced in colors other than ultramarine, and even in this case seems to me arbitrary; neither have I succeeded in framing any explanation in accordance with the theory of Young and Helmholtz which seems plausible.

ART. X.—*On some Phenomena of Binocular Vision;* by JOSEPH LECONTE.

[Read before the National Academy of Sciences, April 20, 1880.]

XI.—*Laws of Ocular Motion.**

IN March, 1869, I published a paper "on the rotation of the eyes on their optic axes, in convergence." The results reached in that paper were briefly as follows:

1. In optic convergence in the *primary* visual plane there is a rotation of both eyes on their optic axes *outward*, and this rotation increases with the degree of convergence.

2. In inclining the visual plane downward the rotation for the same degree of convergence *decreases* until when the inclination is 45° below the primary position, the rotation becomes zero for all degrees of convergence. Below 45° the rotation becomes *inward*.

3. In elevating the visual plane the rotation, for strong convergence, *increases*.

There can be no doubt that the 1st law is plainly in violation of the *Law of Listing* which is supposed to govern all the movements of the eye: for that law requires that all movements of the eye in the primary plane are effected without any rotation on the optic axes (torsion). But it seems not impossible and perhaps not improbable, that the modifications of the effect of convergence in elevating and depressing the visual plane may be the result of the operation of that law; for by that law oblique position upward or downward and to one side or the other does produce rotation. Furthermore, according to Helmholtz, oblique position upward and to one side produces rotation (torsion) to the *opposite* side, and oblique position *downward* and to one side produces torsion to the *same* side. If this be true, then supposing the eyes under the influence of two laws, viz: a law of torsion by convergence, and a law of torsion by oblique position, in elevating and converging the eyes the two would coöperate and produce greater torsion, as indeed we find; and in depressing and converging the eyes, the two would antagonize and neutralize each other, and thus decrease the rotation, as we also find. This seems a simple and satisfactory mode of explaining the whole phenomena of torsion in convergence.

It was in this spirit and the expectation of this result, that I recently undertook a re-investigation of the whole subject of the laws of ocular motion. My first effort was directed to a thorough mastering of the law of Listing; for the statements

* For the other papers on this subject, see this Journal, II, vol. xlvii, pp. 68 and 153, III, vol. i, p. 33, vol. ii, pp. 1, 314, 417, and vol. ix, p. 159.

concerning this law had seemed to me inconsistent with each other. I therefore read again carefully and thoughtfully Helmholtz's great work on Physiological Optics, the acknowledged standard on this subject. I read and re-read several times his chapter on the laws of ocular motion, and pondered upon his results. I repeated all his experiments, and made many more of my own. But so difficult and delusive are experiments of this kind, so beset on every side with sources of fallacy, that the more I experimented and pondered, the more I became bewildered. But now at last the whole subject has become clear, and all my experiments consistent with each other. I now see also, that the true cause of my bewilderment was not so much the delusiveness of the phenomena, but the too ardent desire to verify the results of others, rather than to determine the law for myself. I have been driven almost against my will to the conclusion that there are some strange and apparently inadvertent mistakes in Helmholtz's interpretation of Listing's law, and that this law governs the motions of the eyes only when they *move* parallel to each other, but cannot in any way account for the torsions of the eyes in convergence.

I will now detail the experiments upon which these conclusions are based.

It is well known that spectral images (accidental images of Helmholtz) are the most accurate means of determining the torsions of the eye. They are so because being the result of changes in the retina lasting sometimes a minute or more—being in fact the outward manifestation of images as it were burned into the retina—they must of necessity follow with the greatest exactness all the motions of the eye. There is no other mode of detecting torsions of the eyes, in *parallel motion*. All my experiments, therefore, were made with these images.

Experiment 1.—I darken the experimental room by closing the shutters, but allow the light to enter through a narrow vertical slit between two shutters. I now gaze steadily with head erect on the vertical slit for a minute or so. On turning to the blank white wall I see distinctly a colored vertical spectral image of the slit. I arrange my head if necessary, so that the image is perfectly vertical. If I now turn my eyes (without moving the head) horizontally right and left, the image remains vertical; if I turn my eyes directly up or down, by elevating or depressing the visual plane the image still maintains its vertical position. But if I elevate again the visual plane to the extremest degree, say 40°, and then move the point of sight horizontally as far as possible, say 40° to *the right*, the image is no longer vertical but inclines very decidedly to the right, thus / . If I move my

eyes horizontally to the left, the image inclines equally to the left, thus \ . If, after renewing the spectrum, I now depress the visual plane 40°, and then cause the point of sight to travel 40° to the *right*, the image inclines to the *left*, thus \ ; if the point of sight moves to the extreme left, the image turns to the right, thus / .

In all cases the degree of inclination or torsion of the image increases with the degree of elevation or depression of the visual plane and the amount of lateral excursion of the point of sight, right or left. Also the degree and direction of the torsion of the image will be the same for the same position of the line of sight, however that position may have been reached, whether by two motions along rectangular coördinates, as in the preceding experiments, or by oblique motion from the first or primary position.

Experiment 2.—I next made similar experiments, using a *horizontal*, instead of a vertical image. Such an image may be made in the same way, by means of a horizontal slit in the window. When such an image is thrown on a perpendicular wall with the eyes in the primary position (i. e. with face perpendicular and the eyes looking horizontally) its position is of course horizontal. When the eyes move from side to side horizontally, or up and down vertically, it retains its perfect horizontality. But if the eyes be turned obliquely upward and *to the right*, the image inclines to the left, thus — ; if upward and to the left, the torsion is to the right, thus —. In depressing the plane of sight, movement of the eyes to the right makes the image incline to the right, thus —, while movement to the left, makes it incline to the left, thus —.

The *fact* and the *direction* of the torsion of the images, both vertical and horizontal, are very easily established by the somewhat rough method just described. But if we desire to *measure* the amount of torsion of the image, the wall or other experimental plane must be covered with rectangular coördinates vertical and horizontal. By this means I find that extreme oblique positions produce an inclination of the vertical image on the true vertical of the wall of about 15°, but of the horizontal image on the true horizontals of the wall of only about 5°. There is a reason for this difference, which we explain farther on.

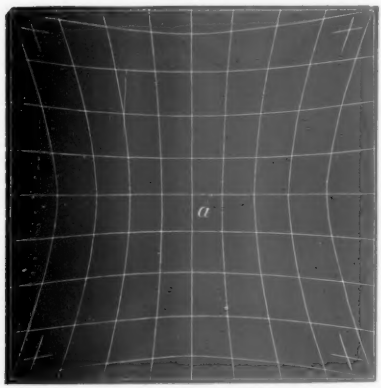
Putting now all these results together, the following diagram (Fig. 1) shows the direction and the degree of inclination of the image for all positions of the point of sight—the center representing the primary position, and the corners extreme oblique positions of the point of sight.

Helmholtz's results are exactly the same as my own, except that he makes the inclination of the vertical and the horizontal image exactly equal, while I find the inclination of the hori-

zontal image much less than that of the vertical image. He embodies his results, therefore, in a diagram like the above, except that the curves of the vertical and the horizontal lines are *exactly* equal each to each in every part of the diagram, while in mine the vertical curves are much greater.

The following diagram plainly shows that the apparent torsion of the vertical and horizontal images are in *opposite* directions. If the inclination or torsion of the images show a corresponding torsion of the eye, the evidence of the two images is contradictory. There must, therefore, be a fallacy somewhere. They both cannot be right; for when one indicates torsion of the eye to the right, the other indicates torsion to the left, and *vice versa*. The vertical and horizontal curves in the diagram are not everywhere at right angles to each other, as they ought to be, if they were both true representatives of ocular torsion. This is best shown by using an image in the form of a rectangular cross.

1.



Experiment 3.—If such an image, made by gazing on a cross slit in the window, be used in the experiments already described, then on turning the eyes obliquely upward and to the right, the cross by the turning of the two parts in opposite directions is distorted, thus +, so that the angles are not all right angles, but 70° and 110°. On turning the eyes upward and to the left, the cross becomes thus +, downward and to the right, thus +, to the left, thus +. The same mode of crossing is observed in the lines of the diagram, and in the crosses in the corners.

It is perfectly evident that this distortion is produced by *projection* of the image on a plane inclined to the line of sight. Helmholtz also attributes this distortion to projection, but he gives no experimental method of eliminating this source of fallacy. If he had done so he would have escaped what I conceive to be the error into which he has inadvertently fallen.

The method which I use to eliminate this source of fallacy is the obvious one of *projecting the image on a plane in every case perpendicular to the line of sight.**

* With *one* eye "*line of sight*" is the proper term—but with two eyes "*median line of sight.*" But except in very near objects the difference is so small that I shall neglect it.

Experiment 4.—For this purpose I prepare a plane a yard square, and cover it with vertical and horizontal lines. In the center I place an upright rod and make it accurately perpendicular to the plane. I place the plane inclined from the perpendicular 30°–40°, and so set that with the face looking straight to the experimental window the plane is 30° or 40° above and to the right: and when I turn my eyes obliquely upward and to the right, I look directly on the end of the rod, so that it is projected as a spot on the plane. I thus know that the line of sight is perpendicular to the plane. Having arranged the plane and my own position to my satisfaction, I gaze on a cross slit in the window until the impression is as it were branded upon the retina, and then turning the eyes obliquely upward and to the right, I throw the image on the center of the plane. The cross image retains perfectly its rectangular symmetry, but is rotated *both parts alike, to the right*, thus, ✕ plainly showing a torsion of the eyes in the same direction. I then make a similar experiment on the left side: the cross turns to the left thus ✕. I now arrange the plane below the head and to the right, but perpendicular to the line of sight when the eyes are turned in that direction. When the image is thrown upon the plane, by turning the eyes obliquely downward and to the right, the cross rotates thus ✕, or when placed on the left, thus ✕. In every case the rectangular symmetry of the cross is perfectly preserved, a sure sign that there is no error by projection.

Experiment 5.—Determined to neglect no means of testing the correctness of these results, I next made experiments in the open air, using the sky as the plane upon which to cast the image. This spatial concave is of course everywhere perpendicular to the line of sight, and therefore eliminates every source of error from projection. Standing with head erect, I gaze on a perpendicular flag-staff until a strong impression is made on the retina. If now holding the head steady, I cast the image on the sky obliquely upward and to the right, the image inclines decidedly to the *right;* if thrown similarly to the left, it inclines to the *left*. With the head in this position, of course the ground prevents making the same experiments with the visual plane depressed. I therefore varied the conditions a little. Sitting on the ground in front of the college building, with the morning sun shining obliquely on its face, the perpendicular light-colored pilasters gleaming in the sunshine, contrast strongly with the shadows which border their northern margin. Gazing steadily on the building, I easily get a strong spectral image of the whole building, with its distinctly-marked vertical and horizontal lines. Now throwing myself

flat on my back, I see the image perfectly erect, in the zenith. Turning now the eyes upward (toward the brows) and to the right and left; and then downward (toward the feet), and to the right and left, the whole image of the building rotates without distortion, precisely as indicated in my previous experiments.

I am perfectly confident then that I am justified in formulating the torsions of the eyes when moving together with their optic axes parallel, thus:

1. When the visual plane is *elevated* and the eyes move to the *right*, they rotate on their optic axes to the *right;* when they move to the *left*, they rotate to the *left.*

2. When the visual plane is *depressed*, then motion to the *right* is accompanied by rotation to the *left*, and motion to the *left* by rotation to the *right.*

3. The degree of rotation increases with the amount of elevation or depression of the visual plane, and of the lateral excursion of the point of sight.

Now, the above laws (1 and 2) concerning the direction of torsion, are precisely the reverse of those given by Helmholtz, and therefore of what I expected to find when I commenced this investigation. I quote from his work on Physiological Optics, French edition, 1867.* This edition was revised, corrected and added to by Helmholtz himself, and by his own statement is not only later but more authoritative than the German.

"When the plane of regard is directed upward, lateral displacement to the *right* makes the eye turn to the *left*, and displacement to the *left* makes it turn to the *right.*

"When the plane of regard is depressed, lateral displacements to the *right* are accompanied with torsion to the *right*, and *vice versa.*

"In other words, when the vertical and lateral angles are both of the *same sign*, the torsion is *negative;* when they are of *contrary signs* the torsion is *positive.*"

The very reverse of every one of these propositions is demonstrably true.

I next set myself to find out how the mistake arose. I find its origin evidently contained in the following statement:

"If we throw a *vertical* image on the wall (supposed to be covered with rectangular coördinates vertical and horizontal), we obtain a rotation in direction contrary to that which we have just seen (in the case of the *horizontal* image). In fact, if one looks upward and to the right, the image *does not turn to the left*, but *to the right in relation to the vertical lines of the wall. But one cannot conclude* from this that there is a *rotation* of the eye

* Optique Physiologique, p. 602 and 603.

to the right, for *in this case the vertical lines of the wall do not coincide with a projection, on the wall, of a perpendicular to the plane of regard. The latter would, on the contrary, appear turned in the same direction as the image, and at an angle much greater than that of the image.*"*

In other words, (since the plane perpendicular to the line of sight is the only true plane of projection, and verticals on that plane are perpendicular to the plane of regard), according to Helmholtz the horizontal lines on the wall are true terms of comparison for determining the rotation of spectral images, because they coincide with the horizontals on the plane perpendicular to the line of sight; but the vertical lines on the wall are not true terms of comparison, because they do not coincide with the verticals in the plane perpendicular to the line of sight. Now, the very reverse is true. It is the *verticals* on the wall which coincide with the verticals in the plane perpendicular to the line of sight or perpendiculars to the plane of regard, and the horizontals on the wall which do not coincide with the horizontals on that plane. Therefore, it is the verticals on the wall which are the true terms of comparison, by which to determine the direction of torsion of the eye, and horizontals which give deceptive results by projection.

As this is a fundamental point, I must pause to make it clear. Suppose, then, one stands in a room before a wall covered with rectangular coördinates, vertical and horizontal. Suppose, farther, such an one surrounded by a spherical wire-cage, constructed of rectangular spherical coördinates, or meridians and parallels, with pole above the head, and eye in the center. Evidently the surface of this spherical concave is everywhere perpendicular to the line of sight, and therefore like the sky, is a proper surface for testing the true direction of rotation of images in every position of the eye. Evidently, also, the meridians and parallels, everywhere at right angles to each other, are the true coördinates with which to compare the spectral images, in order to determine the direction and degree of their rotation. Now the simple question is: how do these meridians and parallels project themselves on the wall, to an observer at the center? How would their shadows be cast by a light at the center? Evidently the meridians would be cast as straight vertical lines, and therefore coincident with the verticals on the wall. But the parallels would be projected not as straight horizontal lines, and therefore not coincident with the horizontals on the wall, but *as hyperbolic curves inclined in the same way as the horizontal image in Helmholtz's diagram, but at much greater angle.* I repeat, then, that the inclination of the vertical image on the vertical lines of the wall gives the true

† Ibid, p. 605.

torsion of the eye, but the inclination of the horizontal image on the horizontal lines of the wall does not give the true torsion of the eye.

There are many other ways of testing the truth of this last proposition and the falsity of the reverse statement of Helmholtz. If we make, as before, a vertical image, and instead of turning the *eyes* upward and to the right, turn the *body* to the right and the *face* upward and cast the image on the extreme right and upper portion of the wall; the vertical image will be projected vertically on the wall, but a horizontal image cast to the same place, in the same way, will be inclined in the same way as in Helmholtz's diagram, but *at much greater angle.* In this case, the eyes are in the primary position, and therefore there is no rotation at all, the inclination of the horizontal image is the result of projection alone.

Without any attempt at mathematical accuracy, the diagram, figure 2, shows the manner in which spherical coördinates would project on a plane perpendicular wall. The crosses in the corners show how a rectangular cross image

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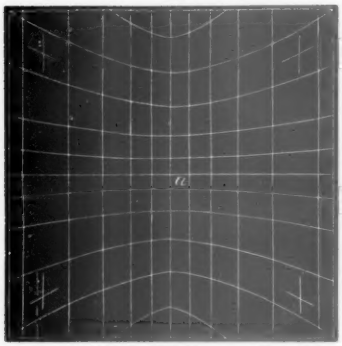

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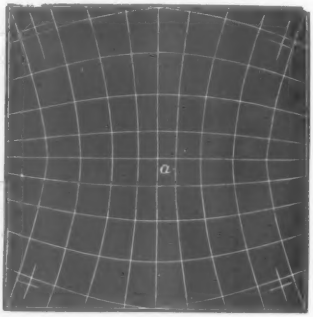
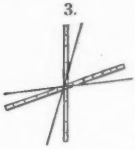


would be distorted by *projection alone.* Now by careful plotting, I have found that at a point 40° upward or downward, and 40° to one side right or left, the inclination of the hyperbolic curves with the true horizontals of the wall is about 20°—which makes the angles of the projected cross 70° and 110°. The rotation of such a cross 15°, would give exactly the results obtained by experiment. In figure 3, the heavy cross shows the position of the image when distorted by projection only, and the lighter lines the same as rotated 15° to the right. As the result of this rotation, the vertical line is inclined 15° *to the right*, while the horizontal line is inclined only 5° *to the left*, as we found by experiment.

3.



Therefore, the diagram which expresses correctly the real torsions of the eyes, in every position of the line of sight, or the diagram which shows the inclination of the vertical and horizontal images, when referred to the meridians and parallels of a spatial concave everywhere perpendicular to the line of sight, is shown in figure 4. In this diagram the lines representing the inclination of the vertical and horizontal images are everywhere at right angles to each other, and always turned in the same direction. By simple inspection of this figure the law of torsion of the cross images and therefore of the eyes in various positions is seen at a glance.

But again, and finally, Helmholtz's statements in regard to the direction of torsion are, it seems to me, in direct contradiction to his own general formulation of Listing's law, taken from Listing himself. This general formula is as follows: "*When the line of regard passes from the primary position to any other position, the angle of torsion of the eye in its second position, is the same as if the eye had come to this second position by turning about a fixed axis perpendicular both to the first and to the second position of the line of regard.*"* Now an axis which satisfies these conditions can be none other than an equatorial axis, i. e., an axis at right angles to the polar or optic axis. In turning from side to side horizontally, it is a *vertical* equatorial axis. In turning up and down, it is a *horizontal* equatorial axis. In turning obliquely, as in the experiments on torsion, it is an *oblique* equatorial axis. Now let any one take a globe, and placing the equator in a vertical plane, make a distinct vertical and horizontal mark across the pole. If now the globe be turned on an oblique equatorial axis so that the pole shall look upward and to the right, it will be seen that the polar cross is no longer vertical and horizontal, but has rotated *to the right*, not the left, as Helmholtz's statements would indicate. Turning of the globe on a fixed axis so that the pole looks upward and to the left, will cause the cross to rotate to the left. So turning downward and to the right, produces rotation to the left, and to the left rotation to the right. If the globe turns thus on an axis inclined 45° to the vertical, and through an arc of 90°, the rotation of the polar cross will be exactly 45°. Thus Listing's law, as understood by himself, is in exact accordance with my results.

I have now, I believe, established on the firm basis of experiment, the true law of torsion of the eyes, when moving parallel to each other. I have also shown that it is identical with Listing's law, properly understood, and I shall therefore continue to call it by that name. Misled by Helmholtz's very positive statements, I commenced this investigation with the expec-

* Op. cit., p. 606.

tation that the operation of Listing's law, when combined with that of convergent motion, would completely explain all the phenomena described in my previous paper. In this expectation I have been disappointed. On the contrary, the investigation brings out in stronger relief than ever before, the *complete contrast* between the two laws. We would thus formulate the contrast:

1. When the eyes move in the *same direction* parallel to each other, in the *primary plane*, there is *no torsion* or rotation on the optic axis; but when they move in the primary plane in *opposite* directions as in convergence, they rotate outward, i. e., toward the temples, thus

2. When the plane of sight is elevated, and the eyes move together parallel to each other, then if the lateral motion is to the right, the rotation is to the right, if to the left, the rotation is to the left; but when in the same position of the visual plane, the eyes move in opposite directions, as in convergence, then as the right eye moves to the left (toward the nose) it rotates to the right, and as the left eye moves to the right (i. e., toward the nose), it rotates to the left. If Listing's law operated at all in convergence, it would tend to neutralize the contrary effect of convergence; but such is not the fact.

3. When the visual plane is *depressed*, the direction of rotation is the same for parallel motion and convergent motion; in both cases the rotation is contrary to the direction of motion. But there is this great difference between the two; by the law of parallel motion, the rotation *increases* with the angle of depression, while by the law of convergent motion, it *decreases* to zero at 45°. If Listing's law operated at all in convergence, it would in this case coöperate and *increase* the motion, but the reverse is the fact, the rotation decreases.

4. There can be no doubt that in oblique motion the vertical and horizontal meridians of the eye become actually inclined on the true vertical and true horizontal, and that if we observed the iris of another person we would see it apparently rotated like a wheel. But although in deference to usage of other writers and to appearance, I have called this change a rotation on the optic axis, yet it seems to me it cannot be properly so called. For all parallel motions of the eyes are rotations on equatorial axes, and therefore on axes in a *plane perpendicular to the polar* or optic axis, and therefore cannot be resolved into rotations on the latter. In parallel rotation, therefore, the so-called torsion is only *apparent and the result of position*, or in other words the result of *reference to a new spatial meridian.* Turning the eye from side to side in the *primary* plane produces no torsion because all the spatial meridians are there *parallel*, but turning from side to side in an *elevated* plane pro-

duces apparent torsion, because the spatial meridians are there *convergent*. But in convergent motion, on the contrary, there is a *real* rotation on the polar or optic axis. This is shown by the fact proven in my previous paper* that one eye, without changing its position, will rotate through the influence of the convergent motion of the other eye.

5. It would seem at first sight, that spectral images might be used also for determining rotations of the eyes in convergence, but they cannot be used for this purpose at all. This brings into view another point of contrast, between convergent and parallel motion. In parallel motion spectral images follow all the motions of the eyes up and down, or to right and left, and all their rotations to one side or the other, with the utmost exactness. In convergent motion, on the contrary, though the eyes may each move through an angle of 45° or more, the position of the spectral image is the same, viz: in front; and though the eyes in extreme convergence may rotate in opposite directions each 10°, yet the spectral image retains its vertical position. The reason of this is that, although there are two retinal brandings, and therefore two spectral images, the external representatives of these brandings, yet the brandings being on corresponding points of the two retinæ, their external representatives, the two spectral images, are indissolubly united. Their separation, either wholly or partially, would be a violation of the law of corresponding points, a law which is never violated under any circumstances whatever.

In conclusion, then, it is evident that when the eyes move in the same direction, parallel to each other, as in ordinary vision of objects, all their motions are governed by the Law of Listing. But when, on the contrary, they move in *opposite* directions, as in strong convergence, then the law of Listing is entirely abrogated or overborne, and another law reigns in its place.

ART. XI.—*Bernardinite: Its Nature and Origin;* by J. M. STILLMAN.

IN a previous number of this Journal† I published the results of a chemical investigation of a resinous substance from San Bernardino, sent to me by Hon. B. B. Redding, which was said to occur in the form of vein in detached masses, and the vein to be traceable for three miles. The finders (farmers or "ranchers" of that vicinity) sent at the same time pieces of rock as vein-stuff which contained this peculiar resinous substance in the crevices. Some months later another specimen was sent to this University from Santa Añia in the same section

* This Journal, II, vol. xlvii, p. 162. † III, vol. xviii, p. 57.

of the country by a resident who stated in his letter that on throwing a match upon the ground he was surprised to see these rocks take fire and burn. He therefore sent a piece to be examined.

The specimens furnished to Mr. Redding were examined by me and the result published in the above mentioned article. The substance, which was extremely light, white and porous, almost chalky, was shown to be mainly a well-marked resin, leaving but a trace of an ash on combustion. No theory was advanced as to its origin, attention simply was called to its structure:—"On fracture it presents a slightly fibrous structure. Under the microscope it exhibits a two-fold structure,—a quantity of very fine, irregular fibers permeating a mass of a brittle, amorphous, structureless substance." Since that paper was written I have endeavored to obtain more definite information as to the origin and occurrence of this peculiar substance. The region of its occurrence is so remote and so inaccessible that it has been impossible for me to investigate the matter in person, and difficult to find competent persons whose business takes them into that region. However, from reports obtained through the agency of Mr. Redding, I feel tolerably confident that the true nature and origin of this substance has been cleared up.

It seems that there grows and probably has grown for a long time a species of conifer which exudes large masses of a resinous secretion from abrasions or wounds. These resinous masses are reported to attain considerable size, and to fall off from their own weight. However that may be, the detached resin either from fallen and decayed trees, or from living trees, becomes scattered over the surface of the country and mixed with surface soil and rocks. By a long process of evaporation, action of atmosphere, and the leeching and bleaching agency of the snow which covers the ground for a large portion of the year, these resinous masses lose all vestiges of volatile and soluble matter and at the same time, a fungus growth permeates and splinters the whole mass into minute fragments rendered coherent by the fibers of the fungus. Hence the two-fold structure noted, the fungus growth as shown in the previous paper, amounting to less than 10 per cent of the mass.

The perfect change which has taken place in the resin by these agencies evidence that the resin must have been exposed for an indefinite period to atmospheric agencies, and have attained a position of equilibrium toward its surrounding conditions. It is therefore apparently entirely a surface formation, which however has in process of time become so mixed in with surface soil and rocks as in some instances to present the appearance of being *in situ*.

University of California, May 13, 1880.

ART. XII.—*Recent Researches on the Lunar Theory;* by JOHN N. STOCKWELL.

IN the November and January numbers of this Journal I have given some account of the inequalities in the moon's motion, arising from the oblateness of the earth. I now propose to give a somewhat detailed account of my investigations into the general theory of the moon's motion as affected by the sun's attraction. Although this problem has undoubtedly received more attention from mathematicians and astronomers, during the past century, than any other arising from the general gravitation of matter, it is nevertheless conceded by those who have given most attention to the subject, that the best lunar theories of the present day are essentially defective and erroneous; and that they signally fail to represent the motions of the moon with a precision at all commensurate with the refinements of calculation.

Early in the year 1876, I called the attention of my friend, the late Leonard Case, to the unsatisfactory state of the lunar theory, and he immediately suggested that I should undertake a thorough and systematic examination of the physical theory of the moon's motion, for the purpose of ascertaining whether the acknowledged defect arose from some oversight committed in the development of the theory, or was due to the omission of some of the smaller terms of the series produced by an otherwise correct development. At the same time Mr. Case, with characteristic generosity, offered to defray all the expenses arising from the prosecution of these researches.

The opportunity thus presented for a thorough investigation of the lunar theory was therefore very cheerfully accepted, although I had some misgivings as to my ability to do justice to a subject which had successfully baffled the best efforts of mathematicians. I had, however, somewhat familiarized myself with some of the methods employed by mathematicians in the treatment of this and similar problems by devoting the little leisure at my command to this subject during a number of years. The subject was therefore quite in harmony with my previous course of reading, and notwithstanding some false steps have been made in the application of a new method of analysis, the results at last obtained are both interesting and satisfactory.

In my investigations thus far I have not, however, attempted to carry on the approximation to terms of so high an order of magnitude depending on the eccentricity and inclination of the orbit, as Delaunay and others have done. I preferred rather to first satisfy myself that no systematic error among terms of the

third or fourth orders of magnitude depending on these quantities had by any means found its way into the calculation; because such errors would vitiate the calculations of the terms of a still higher order. My investigation includes all the terms of perturbation arising from the fourth, and inferior powers of the eccentricity and inclination of the orbits of the sun and moon, while the investigations of Delaunay include the sixth power of these quantities. The general agreement of my work with the results of Delaunay's calculation is, on the whole, quite satisfactory, but there are a few cases in which the results are entirely at variance, even in terms of the third and fourth orders. In this discussion I shall restrict myself to the comparison of those terms in which the agreement is almost perfect, and also to those in which they are most widely different.

I will first give the value of that part of the co-efficient of the inequality which is known by the name of *variation*, which is independent of the eccentricities and inclinations of the orbits. According to my method of development this coefficient is composed of the following terms:

$$2111''\cdot841 - 5''\cdot562 - 0''\cdot030 - 0''\cdot0007 = 2106''\cdot248.$$

According to Delaunay's development, this coefficient is made up of the following terms:

$$1586''\cdot888 + 424''\cdot447 + 80''\cdot091 + 12''\cdot769 + 1''\cdot809 + 0''\cdot223 + 0''\cdot021 = 2106''\cdot248.$$

These two results are identically equal to each other. But a most important distinction between them is the convergency of the series by which they are determined. The four terms of my development are more accurate than seven terms of Delaunay's, since the seventh term of the latter series is thirty times greater than the fourth term of the former.

If we now compare the coefficients of the term whose argument is *twice* the argument of the *variation*, we shall find, according to my development—

$$8''\cdot789 - 0''\cdot056 - 0''\cdot0001 = 8''\cdot733;$$

while Delaunay gives

$$5''\cdot070 + 2''\cdot612 + 0''\cdot813 + 0''\cdot196 + 0''\cdot060 = 8''\cdot751.$$

These two coefficients, though practically equal to each other, show the same remarkable difference in the convergency of the series, the second term of my development being smaller than the fifth of Delaunay's.

For the equation whose argument is three times argument of the variation, I find $0''\cdot0493 - 0''\cdot0005 = 0''\cdot0488$, while Delaunay gives $0''\cdot0218 + 0''\cdot0167 = 0''\cdot0385$.

This coefficient of Delaunay's is about one-fourth part too small, since he has not carried the approximation to terms of so high an order as he did for the two former cases. To show, however, that my coefficient is correct, I would observe that the Monthly Notices of the Royal Astronomical Society for November, 1877, contains a paper by Prof. J. C. Adams, which purports to give the coefficients of the equations we have been comparing, with extreme accuracy. If we reduce his coefficient of $\sin 6(nt-n't)$ to seconds of arc, we obtain $0''{\cdot}0490$ for this coefficient, a value almost identical with my own. For the coefficient of $\sin 8(nt-n't)$ I find $0''{\cdot}00034$, while according to Prof. Adams it is $0''{\cdot}00031$.

According to my development the coefficient of the parallactic inequality is composed of the following terms:

$$84''{\cdot}523+26''{\cdot}801+10''{\cdot}280+3''{\cdot}872=125''{\cdot}476,$$

while Delaunay gives the following series of terms:

$$74''{\cdot}023+34''{\cdot}330+11''{\cdot}885+4''{\cdot}428+1''{\cdot}862+0''{\cdot}712+0''{\cdot}381 \\ =127''{\cdot}621.$$

The coefficient of this inequality is one of the most troublesome to be determined by the theory, and the four terms above given are all I have yet rigorously computed. If we estimate the sum of the remaining terms, by induction from those already calculated, we should increase the preceding coefficient by $2''{\cdot}10$, which would make it equal to $127''{\cdot}58$. Delaunay's coefficient ought also to be increased for the same reason, by about $0''{\cdot}38$, which would make it about $128''{\cdot}00$. These coefficients correspond to a solar parallax of $8''{\cdot}75$. According to my calculations the eccentricity and inclination would diminish this coefficient by $2''{\cdot}11$; and if we assume the mass of the moon to be *one-eightieth* of the earth's mass the perturbations of the earth by the moon would diminish it by $2''{\cdot}10$ more. The theoretical coefficient for the above value of the parallax would therefore be $123''{\cdot}37$. Were the exact value of the coefficient of this inequality determined from observation, we might, by comparing it with the theoretical coefficient, determine the correction to our assumed solar parallax.

The preceding inequalities are the principal ones in which the coefficients of different theories are directly comparable with each other. For those inequalities in which the eccentricity and inclination enter as factors, the value of the coefficient depends, to a certain extent, on the manner in which the arguments of the different equations are measured. In most of the lunar theories the anomalies are measured on the plane of the orbit, while the longitudes are measured on the plane of the ecliptic;—a needless complication, which I have carefully

avoided. However, in order to show the rapid convergency of the series which determine the principal periodic inequalities depending on the eccentricity and inclination of the orbit, I here give the two terms of the coefficient of the evection which I have computed. The first two terms depending on the first power of the eccentricity are as follows:

$$4280''\cdot9+122''\cdot0,$$

while Delaunay gives the following terms:

$$3176''\cdot4+1041''\cdot5+297''\cdot5+72''\cdot3.$$

It is evident that the first series converges about ten times as rapidly as the second.

The preceding comparison is sufficient to show the correctness and value of the method which I have employed in the problem of the moon's motion; and I shall now mention a few cases in which my results are wholly different from what other calculators have found for the same inequalities.

Before doing so, however, I would observe that there are certain fundamental and axiomatic conditions which ought to be satisfied by the results arrived at, whatever be the method of analysis which we may employ. In the present case the condition to be satisfied is simply, *That all the terms introduced into the expressions of the coördinates by the disturbing function ought to disappear when the disturbing function is put equal to nothing.* It is, however, a remarkable fact in connection with the lunar theory, that, among the *four hundred and seventy-nine* equations of the longitude given by Delaunay, there are *five*, arising from the sun's attraction, which do not disappear when the disturbing function is put equal to nothing. From this circumstance it is easy to conclude that there must be something seriously wrong in his development, notwithstanding its intricacy and refinement. The same remark is also applicable to the lunar theories of LaPlace, Plana and Pontécoulant.

The most important of these equations are those having the arguments, $2F - l$, and $D + l'$, in Delaunay's theory; or, *twice the moon's distance from the node minus the mean anomaly*, and *the moon's longitude minus the longitude of the sun's perigee.* The first of these is an inequality of pure elliptic motion, with a coefficient of $+45''\cdot4$, while the coefficient arising from perturbation amounts to $-84''\cdot8$, $28''$ only of which disappears when the disturbing function is put equal to nothing. According to my analysis, the coefficient of this inequality arising from perturbation amounts to only $0''\cdot18$, a quantity less than a *four hundredth part* of Delaunay's coefficient arising from the same cause.

The coefficient of the second equation, mentioned above,

depends entirely on perturbation, and has a value of about 17″ according to Delaunay, while I find a coefficient of only 0″·03. These two equations present the most remarkable differences which I have found among the equations of short period in the moon's motion.

The inequalities of long period, or those which depend wholly on the variation of the elements of elliptical motion are also very easily computed by my method. The values of the inequalities of this kind are subject to very simple and precise laws; so that if we have computed the coefficient of an inequality arising from a given force and having a given period, we may deduce the coefficient of any other inequality arising from a different force and having a different period, directly from it. For convenience we may divide the inequalities of long period into two classes, according to the nature of the forces which produce them. We shall therefore designate the inequalities arising from the variation of the central force as class (A), and those arising from the tangential force as class (B). Then *first*, the inequalities arising from forces of class (A) are to each other as the products of the forces by the periods of their respective arguments; and *second*, the inequalities produced by forces of class (B) are to each other as the products of the forces by the squares of the periods of their arguments.

We may also easily obtain the inequalities produced by either class of forces from the inequalities produced by the other class. For example, suppose the inequalities of the central force f having a period α, is found by calculation to produce an inequality m, in the moon's longitude, and we wish to obtain the inequality produced by a tangential force f', having a period α'. If we call this second inequality m', I find the following relation exists between the two inequalities:

$$3mf'\alpha'^2 = -2m'f\alpha n.$$

This gives $m' = -\frac{3}{2}m\frac{f'}{f}\frac{\alpha'^2}{\alpha n}$, n here denoting the moon's period of revolution. If $f = f'$ and $\alpha = \alpha' = 118{\cdot}3n$, which corresponds nearly to the period of the moon's perigee, we find

$$m' = -178m,$$

whence it follows that for equal central and tangential forces having a period of about nine years, the tangential force would diminish the moon's longitude *one hundred and seventy-eight* times as much as the central force would increase it, and *vice versa*.

There are two inequalities of long period in the moon's motion which have been much discussed by astronomers. They have for arguments, *twice the difference of longitude of perigee*

and node of the lunar orbit, and *the difference of longitudes of perigee of sun and moon*, respectively. Plana was the first to give a correct approximate solution of the problem of the first of these inequalities, which is produced wholly by the variations of the central force. By means of a laborious investigation, occupying about fifty pages of his Theory of the Moon's Motion, he has obtained a tolerably correct approximation to the value of the inequality. He obtains $+1''\cdot405$ for the sum or the elliptic and perturbed coefficient; but the elliptic coefficient is equal to $-0''\cdot932$; whence it follows that the coefficient due to perturbation amounts to about $+2''\cdot34$. I obtain, almost without labor, $+2''\cdot54$ for the value of this coefficient.

The second inequality is produced by both classes of forces, and the determination of its coefficient is more complicated than that of the inequality just mentioned. The value of the force of class (A), which produces the inequality, is about *one-fifth* of the former, but it has a period about *three times* as long. The inequality produced by this force ought to be about *three-fifths* of the former inequality, which would make it equal to $1''\cdot52$. But the tangential force is far more effective, since the inequalities produced are proportional to the squares of the periods of the arguments. I find, however, by an exact calculation that the part of the coefficient of this inequality which arises from the central force amounts to $+1''\cdot45$; while the part of it which arises from tangential force amounts to $+107''\cdot08$; thus making the coefficient of the inequality equal to $108''\cdot53$. The solutions of Plana, Pontécoulant and Delaunay, all make the coefficient equal to about $0''\cdot4$, when quantities of the same order only are included.

It is remarkable that the inequalities of long period arising from the two classes of forces which produce them should follow the same law as the acquired velocity, and space passed over, by falling bodies at the surface of the earth, the one being proportional to the time and the other to the square of the time.

If we extend the comparison to the variation of the elements, we shall find that the method which I have employed possesses the advantage of more rapid convergency. For example, I find for the first two terms of the mean motion of the perigee the following value:

$$0\cdot00419643 + 0\cdot00395575 = 0\cdot00815218,$$

while the first three terms of Delaunay's series are

$$0\cdot00419643 + 0\cdot00294279 + 0\cdot00099570 = 0\cdot00813492.$$

This comparison shows that two terms of my series are considerably more accurate than three terms of Delaunay's.

The preceding comparisons are sufficient to establish two

points in regard to the lunar theory. The first is, that the general methods of computation are undoubtedly correct; and the second is, that one or more of the methods have been incorrectly applied to the investigation of particular inequalities. Now, without claiming that there are no mistakes, either systematic or accidental, in my work on the lunar theory, there are some reasons for believing that it is correct in the cases to which I have called attention. One of these reasons is the fact that all the inequalities produced by perturbation would disappear from the formulas by simply putting the disturbing function equal to nothing; whereas there are a number of inequalities which do not disappear from the formulas of previous investigators by means of the same conditions.

It might seem, however, that such large changes in the values of the coefficients of some of the equations of the moon's longitude, as my researches seem to indicate, would have a tendency to make the theory less accordant with observations than it is at present, since the present lunar tables represent the moon's place within tolerably narrow limits. But a little consideration will show that such a conclusion would not necessarily follow. In order to illustrate this point, let us suppose that we have a perfect system of elements of the moon's orbit together with a perfect theory of the perturbations. It would necessarily follow that the moon's place could be perfectly predicted, and there would be no discordances between theory and observation. Suppose, now, that we omit a number of small though important equations from the computation of our ephemeris, it would follow that there would be a series of residuals between theory and observation. It is evident that these residuals would be perfectly represented by the omitted equations; but if the equations were considered as wholly lost, the theory would be in the same condition as though they had never been found; and we might seek to make up for the imperfect theory by finding certain corrections to the elements by means of equations of condition between the variations of the elements and the observed residuals. In this way we might perhaps obtain a very good agreement between theory and observations which extend over a limited interval of time,—the errors of the theory being partially compensated by the errors of the elements. But this close agreement between theory and observation would soon cease to take place, since the corrections applied to the elements would vitiate the remaining part of the theory. The imperfect ephemeris, computed by means of the changed elements and theory, would gradually depart more and more widely from the observed place of the moon, but the residuals would furnish no information in regard to the nature of the equations to be applied in order to correct them,

since the calculated places were based on imperfect elements and an imperfect theory of the perturbations.

Now it seems to me that the actual history of the lunar theory indicates a passage through just such conditions and changes. It is true, however, that it has never possessed the advantages of perfection assumed above; but through the efforts of astronomers to improve the accuracy of the elements and tables, it has been subjected to the same process of correction. Beginning with Tycho Brahe, in modern times, the elements and theory of the moon's motion were so imperfect that the observed discordances forced him to recognize the existence of two considerable inequalities which were at the time unknown to the science of Europe. The discovery of these inequalities, which have received the names of *variation* and *annual equation*, was the last great step towards the perfection of the lunar theory, which preceded the discovery of the physical cause of the inequalities. Since that memorable epoch, the researches of mathematicians have instructed observers in regard to the magnitude and laws of numerous inequalities which must necessarily affect the moon's motion. The labors of Newton and Halley reduced the errors of the theory to about the *eighth* part of a degree; while Mayer, by the aid of theory and more accurate observations, succeeded in reducing the errors to less than the *thirtieth* part of a degree. Later still, the researches of Mason and Burg, according to the authority of LaPlace, reduced the errors of the theory to less than *one-quarter of a minute of arc!* If this last degree of precision was at any time really attained, it must have been owing to the partial compensation of errors of theory by the errors of the elements; because the theory very soon began to depart more and more widely from the observations; and astronomers have been obliged to suspect that the moon's motion was affected by one or more equations of very long period, which theory is hopelessly unable to point out.

About the middle of the present century, new tables of the moon's motion were constructed by Hansen; and, considering the broad basis of observation and the elaborately developed theory of her motion, the hope was justified that the elements and theory were so perfectly known, that they would permanently represent the observations. But this hope seems to have been ill founded. In a very few years the observations unmistakably indicated a growing discordance, which has continued till the present time; and notwithstanding the laborious investigations which have been made in order to detect the laws and the cause, no satisfactory explanation has yet been attained. If we consider the nature and magnitude of the discordances which now pertain to the best tables of the moon's

motion, we can hardly avoid the conclusion that they are not due to the terms of a higher order of magnitude which have been neglected in the development of the theory. They must therefore result from some systematic error among terms of more importance in the lunar theory.

In bringing to a close this account of my researches, I would repeat that my only object has been to discover if possible, by means of a new method of investigation, any false steps which may have been committed by previous investigators in the mathematical development of the lunar theory. The history of philosophy affords numerous examples of the advantages of independent methods of investigation over independent calculations by the same method. It often happens that for particular values of the known quantities of a problem some terms of the solution become infinite or indeterminate, when certain general methods of investigation are employed; whereas other methods would not be subject to complications from such a cause. It is therefore evident that the comparison of the results of different methods would serve to call attention to the particular terms affected by any such critical conditions, and by thus narrowing the field of investigation, enable astronomers to concentrate their efforts on those particular terms where further research would seem to be necessary or desirable; and it is believed that the terms to which I have called attention afford the means of a much needed improvement in the lunar theory.

Cleveland, May 25, 1880.

ART. XIII.—*Aqueous Vapor in Relation to Perpetual Snow;* by JAMES CROLL, LL.D., F.R.S.*

SOME twelve years ago I gave (Phil. Mag., March, 1867, Climate and Time, p. 548) what appears to be the true explanation of that apparently paradoxical fact observed by Mr. Glaisher that the difference of reading, between a thermometer exposed to direct sunshine and one shaded, *diminishes* instead of increases, as we ascend in the atmosphere. This led me to an important conclusion in regard to the influence of aqueous vapor on the melting of snow, but recent objections to some of my views convince me that I have not given to that conclusion the prominence it deserves. I shall now state in a few words the conclusion to which I refer.

The reason why snow at great elevations does not melt but remains permanent, is owing to the fact that the heat received from the sun is thrown off into stellar space so rapidly by radi-

* Communicated to this Journal by the author.

ation and reflection that the sun fails to raise the temperature of the snow to the melting point; the snow evaporates but it does not melt. The summits of the Himalayas, for example, must receive more than ten times the amount of heat necessary to melt all the snow that falls on them, notwithstanding which the snow is not melted. And in spite of the strength of the sun and the dryness of the air at these altitudes, evaporation is insufficient to remove the snow. At low elevations, where the snow-fall is probably greater, and the amount of heat received even less than at the summits the snow melts and disappears. This, I believe, we must attribute to the influence of aqueous vapor. At high elevations the air is dry and allows the heat radiated from the snow to pass into space, but at low elevations a very considerable amount of the heat radiated from the snow is absorbed by the aqueous vapor which it encounters in passing through the atmosphere. A considerable portion of the heat thus absorbed by the vapor is radiated back on the snow, but the heat thus radiated being of the same quality as that which the snow itself radiates, is on this account absorbed by the snow. Little or none of it is reflected like that received from the sun. The consequence is that the heat thus absorbed accumulates in the snow till melting takes place. Were the amount of aqueous vapor possessed by the atmosphere sufficiently diminished, perpetual snow would cover our globe down to the sea-shore. It is true that the air is warmer at the lower level than at the higher level and by contact with the snow must tend to melt it more at the former than at the latter position. But we must remember that the air is warmer mainly in consequence of the influence of aqueous vapor, and that were the quantity of vapor reduced to the amount in question, the difference of temperature at the two positions would not be great.

But it may be urged as a further objection to the foregoing conclusion, that as a matter of fact on great mountain-chains, the snow-line reaches to a lower level on the side where the air is moist than on the opposite side where it is dry and arid. As, for example, on the southern side of the Himalayas and on the eastern side of the Andes where the snow-line descends some 2,000 or 3,000 feet below that of the opposite, or dry side. But this is owing to the fact that it is on the moist side that by far the greatest amount of snow is precipitated. The moist winds of the S. W. monsoon deposit their snow almost wholly on the southern side of the Himalayas, and the S. E. trades, the snow on the east side of the Andes. Were the conditions in every respect the same on both sides of the mountain ranges with the exception only that the air on one side was perfectly dry, allowing radiation from the snow to pass without interruption into stellar space, while on the other side the air was moist

and full of aqueous vapor, absorbing the heat radiated from the snow, the snow-line would, in this case, undoubtedly descend to a lower level on the dry than on the moist side. No doubt more snow would be evaporated off the dry than off the moist side, but melting would certainly take place at a greater elevation on the moist than on the dry side, and this is what would mainly determine the position of the snow line.

In like manner the dryness of the air will, in a great measure, account for the present accumulation of snow and ice on Greenland and on the Antarctic continent. I have shown on former occasions that those regions are completely covered with perpetual snow and ice, not because the quantity of snow falling on them is great, but because the quantity melted is small. And the reason why the snow does not melt is not because the amount of heat received during the year is not equivalent to the work of melting the ice, but mainly because of the dryness of the air, the snow is prevented from rising to the melting point.

There is little doubt but that the cold of the glacial epoch would produce an analogous effect on temperate regions to that experienced at present on Arctic and Antarctic regions. The cold, although it might, to some extent, diminish the snow fall, would dry the air and prevent the temperature of the snow rising to the melting point. It would not prevent evaporation taking place over the ocean by the sun's heat, but the reverse, but it would prevent the melting of the snow on the land during the greater part of the year.

In places like Fuego and South Georgia, where the snow fall is considerable, perennial snow and ice are produced by diametrically opposite means, as I have elsewhere shown, viz: by the sun's heat being cut off by clouds and dense fogs. In the first place the upper surfaces of the clouds act as reflectors, throwing back the sun's rays into stellar space, and in the second place, of the heat which the clouds and fogs absorb, more than one-half is not radiated downward on the snow, but upward into space. And the comparatively small portion of heat which manages to reach the ground and be available in melting the snow is insufficient to clear off the winter's accumulation.

Art. XIV.—*Perihelion and Eccentricity;* by R. W. McFarland. With Plate III.

The following table gives the longitude of the perihelion and the eccentricity of the earth's orbit for a period of 4,520,000 years, of which 3,260,000 are before 1850, and 1,260,000 after 1850. It is computed by the formulæ of LeVerrier, as quoted

in Mr. Croll's "Climate and Time," and also by those given in Mr. Stockwell's pamphlet on the "Secular Equations of the Moon's Mean Motion." The constants of the latter formulæ differ slightly from those found for the sun's increased parallax in the xviiith vol. of the Smithsonian Contributions.

An inspection of the table shows that the motion of the perihelion is exceedingly irregular, and occasionally retrograde.

The intervals between the maximum and minimum points in the curve of eccentricity also vary greatly, as a glance at the chart will show. The initial periods, 1800 and 1850, for the two sets of formulæ, differ so little from each other that the ordinates to the curves practically coincide.

One object of the table is a comparison of the results reached by these two sets of formulæ. It is obvious that great changes in the eccentricity will occur, unless the elements of disturbance reduce more nearly to zero than is probable, or perhaps possible.

The chart also shows that the *time*, rather than the value of the maximum eccentricity varies; but even this variation of the time, as shown by the two series of values, is not great when long periods are considered.

Mr. Stockwell's formulæ are deemed the more accurate, yet the two curves exhibit a general conformity throughout their whole extent. Whether a period of high eccentricity occurred about 800,000 years ago, or 700,000, is a small matter:—the chief point of interest is, that such a period has actually been found in the past. For about 70,000 years, partly on each side of 1850, the curves differ but slightly.

It is regretted that it has been necessary to draw the curves on so small a scale. When drawn large they move on with a generous sweep, so to speak, and make no sharp turns.

The computations having been made in the small intervals of leisure amid the press of onerous duties, errors may have been made and remained undetected; but it is thought that there are none of sufficient magnitude to vary the general results to any considerable amount.

The computations were originally begun at the instance of President Orton, of the Ohio State University, and were continued, I may be permitted to say, on the suggestion of Mr. James Croll, of the Geological Survey of Scotland.

It is scarcely necessary to add that if any one wishes to determine the difference between the greatest and least distance of the sun, it is only necessary to multiply the sun's mean distance by twice the eccentricity.

Ohio State University, June 3, 1880.

TABLE *showing the Longitude of the Perihelion and the Eccentricity of the Earth's Orbit for* 4,520,000 *years, according to the Formulas of Stockwell and of Le Verrier.*

Year.	Stockwell.		LeVerrier.		Year.	Stockwell.		LeVerrier.	
Before 1850.	Long.	Ecc.	Long.	Ecc.	Before 1850.	Long.	Ecc.	Long.	Ecc.
3,260,000	130° 48′	0·0098	235° 7′	0·0408	2,800,000	270° 13′	0·0329	339° 16′	0·0353
3,250,000	187 18	0·0136	261 22	0·0446	2,790,000	313 35	0·0407	13 19	0·0440
3,240,000	226 22	0·0153	284 34	0·0454	2,780,000	351 58	0·0460	47 40	0·0477
3,230,000	261 39	0·0152	308 13	0·0435	2,770,000	27 39	0·0474	81 13	0·0466
3,220,000	295 49	0·0112	329 26	0·0388	2,760,000	61 48	0·0445	120 4	0·0409
3,210,000	336 10	0·0072	336 38	0·0340	2,750,000	95 3	0·0373	161 22	0·0326
3,200,000	104 18	0·0023	356 34	0·0251	2,740,000	128 5	0·0266	211 32	0·0243
3,190,000	198 8	0·0121	353 48	0·0220	2,730,000	163 8	0·0181	274 41	0·0203
3,180,000	225 37	0·0225	350 24	0·0256	2,720,000	335 32	0·0020	335 0	0·0234
3,170,000	258 39	0·0330	0 35	0·0332	2,710,000	32 44	0·0165	25 30	0·0286
3,160,000	290 33	0·0416	21 1	0·0422	2,700,000	111 34	0·0299	65 35	0·0329
3,150,000	321 57	0·0469	45 34	0·0499	2,690,000	97 48	0·0420	101 36	0·0342
3,140,000	352 46	0·0477	74 17	0·0528	2,680,000	128 58	0·0504	136 1	0·0315
3,130,000	22 23	0·0435	103 13	0·0518	2,670,000	161 29	0·0537	171 12	0·0254
3,120,000	49 47	0·0346	134 22	0·0462	2,660,000	193 57	0·0537	211 36	0·0142
3,110,000	70 38	0·0222	166 45	0·0370	2,650,000	227 53	0·0479	318 37	0·0053
3,100,000	50 12	0·0120	200 38	0·0253	2,640,000	263 45	0·0385	54 24	0·0173
3,090,000	17 29	0·0173	237 50	0·0124	2,630,000	304 46	0·0264	93 36	0·0331
3,081,000	----	----	319 39	0·0018	2,620,000	3 15	0·0143	127 6	0·0478
3,080,000	30 28	0·0319	5 34	0·0015	2,610,000	104 16	0·0136	158 59	0·0592
3,079,000	----	----	43 6	0·0021	2,600,000	172 4	0·0221	190 3	0·0660
3,070,000	55 19	0·0450	113 29	0·0115	2,590,000	215 14	0·0342	220 29	0·0666
3,060,000	82 45	0·0524	153 41	0·0202	2,580,000	257 22	0·0383	249 56	0·0609
3,050,000	101 54	0·0560	192 57	0·0264	2,570,000	297 34	0·0411	277 29	0·0492
3,040,000	140 3	0·0580	233 17	0·0303	2,560,000	339 17	0·0397	299 44	0·0330
3,030,000	168 16	0·0523	274 50	0·0326	2,550,000	24 15	0·0369	298 34	0·0167
3,020,000	194 49	0·0427	316 55	0·0341	2,540,000	74 21	0·0323	253 58	0·0192
3,010,000	218 35	0·0302	359 0	0·0355	2,530,000	78 17	0·0252	259 26	0·0368
3,000,000	241 23	0·0151	39 29	0·0365	2,520,000	181 2	0·0321	283 4	0·0535
2,990,000	198 18	0·0095	77 59	0·0365	2,510,000	226 46	0·0344	310 17	0·0657
2,980,000	175 47	0·0181	114 16	0·0351	2,500,000	268 54	0·0352	338 37	0·0721
2,970,000	167 19	0·0295	148 37	0·0315	2,490,000	307 39	0·0331	7 18	0·0722
2,960,000	218 8	0·0387	181 1	0·0260	2,480,000	345 17	0·0275	35 46	0·0662
2,950,000	246 23	0·0439	210 39	0·0170	2,470,000	23 59	0·0189	63 26	0·0553
2,940,000	277 31	0·0467	247 45	0·0033	2,460,000	81 48	0·0083	88 42	0·0414
2,935,000	----	----	178 9	0·0027	2,450,000	212 16	0·0091	109 34	0·0252
2,930,000	310 28	0·0443	125 28	0·0067	2,440,000	266 21	0·0217	97 19	0·0114
2,920,000	345 49	0·0386	142 5	0·0198	2,430,000	302 25	0·0343	57 27	0·0138
2,910,000	26 14	0·0320	170 58	0·0343	2,420,000	334 51	0·0455	64 45	0·0253
2,900,000	80 3	0·0220	200 50	0·0443	2,410,000	353 37	0·0537	88 41	0·0351
2,890,000	150 12	0·0194	231 52	0·0529	2,400,000	36 3	0·0571	116 40	0·0415
2,880,000	214 49	0·0219	263 28	0·0574	2,390,000	65 41	0·0560	147 43	0·0441
2,870,000	266 36	0·0278	295 22	0·0599	2,380,000	95 31	0·0499	181 21	0·0431
2,860,000	313 3	0·0317	327 37	0·0518	2,370,000	128 32	0·0423	218 32	0·0388
2,850,000	353 45	0·0319	0 18	0·0416	2,360,000	143 3	0·0243	260 0	0·0334
2,840,000	38 42	0·0293	30 16	0·0265	2,350,000	130 24	0·0106	308 22	0·0281
2,830,000	4 16	0·0247	71 24	0·0110	2,340,000	78 26	0·0179	2 30	0·0254
2,825,000	----	----	114 24	0·0028	2,330,000	95 38	0·0342	57 16	0·0257
2,824,000	----	----	154 20	0·0017	2,320,000	121 34	0·0485	106 9	0·0270
2,822,500	----	----	215 56	0·0033	2,310,000	150 25	0·0585	151 18	0·0268
2,820,000	154 24	0·0216	261 28	0·0065	2,300,000	179 52	0·0620	195 27	0·0238
2,810,000	217 32	0·0253	305 41	0·0226	2,290,000	201 24	0·0611	295 49	0·0181

Table continued—Perihelion and Eccentricity.

Year.	Stockwell.		LeVerrier.		Year.	Stockwell.		LeVerrier.	
Before 1850.	Long.	Ecc.	Long.	Ecc.	Before 1850.	Long.	Ecc.	Long.	Ecc.
2,280,000	239 47	0·0528	311 19	0·0125	1,730,000	340 35	0·0140	110 43	0·0246
2,270,000	269 45	0·0401	37 55	0·0142	1,720,000	340 0	0·0180	128 20	0·0163
2,260,000	306 38	0·0187	98 1	0·0231	1,710,000	352 37	0·0250	126 44	0·0090
2,250,000	308 17	0·0093	141 18	0·0328	1,700,000	13 5	0·0318	94 40	0·0095
2,240,000	220 25	0·0088	178 39	0·0406	1,690,000	36 31	0·0366	73 31	0·0132
2,230,000	232 54	0·0210	213 22	0·0448	1,680,000	60 33	0·0383	114 59	0·0152
2,220,000	259 19	0·0305	245 55	0·0457	1,670,000	87 53	0·0365	138 51	0·0144
2,210,000	288 41	0·0357	277 20	0·0421	1,660,000	101 41	0·0328	161 51	0·0099
2,200,000	325 37	0·0425	307 7	0·0352	1,650,000	113 45	0·0283	167 54	0·0034
2,190,000	351 5	0·0343	334 20	0·0259	1,640,000	118 32	0·0264	67 45	0·0060
2,180,000	25 2	0·0288	355 28	0·0153	1,630,000	123 11	0·0291	94 30	0·0146
2,170,000	63 45	0·0218	344 38	0·0067	1,620,000	134 45	0·0344	109 4	0·0223
2,160,000	113 23	0·0152	295 35	0·0096	1,610,000	152 16	0·0397	134 23	0·0277
2,150,000	180 38	0·0126	305 22	0·0191	1,600,000	172 52	0·0430	158 42	0·0305
2,140,000	296 41	0·0155	331 51	0·0255	1,590,000	194 36	0·0438	180 56	0·0305
2,130,000	289 22	0·0200	0 5	0·0304	1,580,000	216 8	0·0419	199 43	0·0284
2,120,000	327 5	0·0227	30 37	0·0329	1,570,000	236 26	0·0379	213 12	0·0256
2,110,000	2 48	0·0231	61 37	0·0333	1,560,000	254 20	0·0325	225 26	0·0217
2,100,000	37 42	0·0199	98 54	0·0298	1,550,000	268 17	0·0269	225 47	0·0239
2,090,000	74 40	0·0136	137 46	0·0266	1,540,000	277 2	0·0197	233 17	0·0270
2,080,000	133 32	0·0048	182 4	0·0223	1,530,000	278 26	0·0203	245 34	0·0316
2,070,000	282 13	0·0081	232 10	0·0191	1,520,000	281 28	0·0216	262 9	0·0364
2,060,000	326 55	0·0190	284 54	0·0175	1,510,000	294 19	0·0265	281 54	0·0399
2,050,000	2 1	0·0291	334 45	0·0169	1,500,000	309 16	0·0288	303 28	0·0430
2,040,000	34 8	0·0366	20 50	0·0155	1,490,000	331 51	0·0310	326 52	0·0434
2,030,000	64 56	0·0405	63 11	0·0116	1,480,000	348 5	0·0314	351 32	0·0422
2,020,000	95 46	0·0400	113 0	0·0066	1,470,000	29 6	0·0277	16 1	0·0361
2,013,000	----	----	214 46	0·0030	1,460,000	64 31	0·0224	39 23	0·0285
2,010,000	123 8	0·0352	263 1	0·0045	1,450,000	110 50	0·0152	57 12	0·0195
2,000,000	147 16	0·0273	323 44	0·0139	1,440,000	178 7	0·0097	56 58	0·0119
1,990,000	162 0	0·0182	1 30	0·0249	1,430,000	264 46	0·0117	34 7	0·0130
1,980,000	149 53	0·0126	34 44	0·0328	1,420,000	313 50	0·0175	41 32	0·0200
1,970,000	135 1	0 0165	65 33	0·0363	1,410,000	355 46	0·0215	64 12	0·0248
1,960,000	146 6	0·0236	94 18	0·0427	1,400,000	34 49	0·0222	97 35	0·0315
1,950,000	167 23	0·0299	120 32	0·0427	1,390,000	82 31	0·0188	130 43	0·0325
1,940,000	196 46	0·0331	144 21	0·0399	1,380,000	124 10	0·0134	171 11	0·0336
1,930,000	216 49	0·0326	161 18	0·0356	1,370,000	202 27	0·0089	212 0	0·0323
1,920,000	241 27	0·0311	173 10	0·0320	1,360,000	288 47	0·0138	254 51	0·0309
1,910,000	263 47	0·0268	180 31	0 0311	1,350,000	338 48	0·0233	297 10	0·0283
1,900,000	285 42	0·0215	186 29	0·0335	1,340,000	16 58	0·0322	338 37	0·0255
1,890,000	303 28	0·0162	200 34	0·0381	1,330,000	51 10	0·0383	17 42	0·0216
1,880,000	314 34	0·0114	216 11	0·0428	1,320,000	83 11	0·0412	53 59	0·0159
1,870,000	315 24	0·0082	234 1	0·0465	1,310,000	111 27	0·0434	83 22	0·0082
1,860,000	307 49	0·0074	252 51	0·0490	1,300,000	142 12	0·0347	0 37	0·0025
1,850,000	310 11	0·0084	272 15	0·0500	1,290,000	168 15	0·0266	346 20	0·0123
1,840,000	326 26	0·0095	291 44	0·0492	1,280,000	187 17	0·0164	18 27	0·0226
1,830,000	351 46	0·0095	310 47	0·0466	1,270,000	171 4	0·0069	47 42	0·0327
1,820,000	28 24	0·0082	328 36	0·0425	1,260,000	126 50	0·0127	76 55	0·0412
1,810,000	90 35	0·0069	343 38	0·0377	1,250,000	138 21	0·0233	105 34	0·0474
1,800,000	151 0	0·0078	354 49	0·0334	1,240,000	162 37	0·0339	134 24	0·0508
1,790,000	204 17	0·0110	2 53	0·0314	1,230,000	189 52	0·0419	162 36	0·0508
1,780,000	244 36	0·0150	11 37	0·0331	1,220,000	218 54	0.0465	190 9	0·0473
1,770,000	278 57	0·0179	25 31	0·0346	1,210,000	249 17	0·0472	216 35	0·0395
1,760,000	306 35	0·0188	43 37	0·0363	1,200,000	281 5	0·0439	239 34	0·0289
1,750,000	329 29	0·0175	65 25	0·0352	1,190,000	314 51	0·0366	249 54	0·0168
1,740,000	342 17	0·0149	88 21	0·0312	1,180,000	352 16	0·0257	216 1	0·0121

Table continued—Perihelion and Eccentricity.

Year.	Stockwell.		LeVerrier.		Year.	Stockwell.		LeVerrier.	
Before 1850.	Long.	Ecc.	Long.	Ecc.	Before 1850.	Long.	Ecc.	Long.	Ecc.
1,170,000	42 34	0·0145	204 24	0·0228	610,000	359 20	0·0224	348 54	0·0353
1,160,000	140 15	0·0088	217 5	0·0325	600,000	44 9	0·0353	32 43	0·0418
1,150,000	222 12	0·0167	250 27	0·0473	590,000	81 44	0·0437	72 46	0·0451
1,140,000	269 14	0·0259	280 5	0·0541	580,000	114 53	0·0509	109 42	0·0478
1,130,000	310 0	0·0323	311 18	0·0558	570,000	151 44	0·0535	145 47	0·0450
1,120,000	350 23	0·0376	343 52	0·0522	560,000	185 16	0·0496	179 9	0·0368
1,110,000	32 58	0·0342	17 59	0·0434	550,000	218 19	0·0409	216 47	0·0262
1,100,000	79 32	0·0315	55 21	0·0311	540,000	251 0	0·0283	267 19	0·0126
1,090,000	131 24	0·0293	103 7	0·0177	531,500	----	---	350 27	0·0019
1,080,000	185 42	0·0297	195 34	0·0099	531,000	----	----	18 21	0·0015
1,070,000	236 27	0·0326	279 57	0·0173	530,800	----	----	29 39	0·0016
1,060,000	281 17	0·0360	322 16	0·0243	530,500	----	----	37 31	0·0018
1,050,000	321 18	0·0375	4 10	0·0346	530,000	283 0	0·0133	61 33	0·0024
1,040,000	358 41	0·0366	40 23	0·0340	520,000	136 52	0·0023	126 41	0·0165
1,030,000	34 10	0·0303	76 40	0·0311	510,000	167 44	0·0169	162 49	0·0315
1,020,000	70 11	0·0214	118 17	0·0246	500,000	199 31	0·0287	193 59	0·0388
1,010,000	111 41	0·0096	171 42	0·0172	490,000	228 9	0·0395	226 12	0·0446
1,000,000	271 11	0·0056	248 26	0·0151	480,000	266 20	0·0425	257 51	0·0470
990,000	326 55	0·0197	313 42	0·0224	470,000	294 4	0·0437	290 35	0·0443
980,000	1 14	0·0338	357 59	0·0330	460,600	326 7	0·0410	322 46	0·0387
970,000	32 19	0·0461	34 3	0·0424	450,000	359 48	0·0353	356 51	0·0308
960,000	62 57	0·0535	66 46	0·0491	440,000	35 21	0·0276	34 44	0·0218
950,000	96 23	0·0605	97 47	0·0517	430,000	77 4	0·0191	101 31	0·0132
940,000	125 7	0·0616	127 42	0·0495	420,000	134 45	0·0119	164 17	0·0087
930,000	152 57	0·0548	156 7	0·0423	410,000	215 12	0·0103	240 38	0·0121
920,000	181 54	0·0440	181 41	0·0305	400,000	279 21	0·0145	289 30	0·0167
910,000	210 42	0·0296	194 15	0·0156	390,000	327 5	0·0182	332 14	0·0199
900,000	218 35	0·0115	135 7	0·0109	380,000	10 42	0·0199	14 11	0·0202
890,000	134 27	0·0124	129 36	0·0277	370,000	55 29	0·0206	62 23	0·0185
880,000	146 56	0·0308	152 33	0·0457	360,000	133 37	0·0188	120 14	0·0171
870,000	174 21	0·0472	180 23	0·0608	350,000	164 15	0·0190	182 50	0·0195
860,000	204 33	0·0592	209 41	0·0709	340,000	222 8	0·0222	236 22	0·0260
850,000	234 52	0·0625	239 28	0·0747	330,000	272 6	0·0278	279 56	0·0337
840,000	267 33	0·0649	269 15	0·0717	320,000	314 55	0·0334	317 31	0·0399
830,000	299 43	0·0582	298 27	0·0623	310,000	352 23	0·0370	5 14	0·0430
820,000	333 10	0·0464	325 45	0·0471	300,000	27 08	0·0373	23 26	0·0424
810,000	8 12	0·0309	348 27	0·0297	290,000	58 14	0·0337	47 16	0·0342
800,000	51 25	0·0140	343 48	0·0132	280,000	86 44	0·0262	74 6	0·0286
790,000	193 20	0·0062	293 27	0·0171	270,000	104 43	0·0163	82 35	0·0214
780,000	261 50	0·0196	303 43	0·0325	260,000	83 2	0·0093	63 27	0·0171
770,000	299 45	0·0310	328 40	0·0455	250,000	56 44	0·0161	58 40	0·0260
760,000	334 47	0·0382	357 14	0·0540	240,000	71 51	0·0271	76 53	0·0362
750,000	9 21	0·0410	28 13	0·0580	230,000	94 20	0·0370	96 37	0·0477
740,000	45 54	0·0391	58 31	0·0562	220,000	119 35	0·0437	120 43	0·0545
730,000	84 47	0·0350	90 56	0·0505	210,000	144 46	0·0471	138 16	0·0567
720,000	127 6	0·0290	125 5	0·0419	200,000	169 0	0·0470	168 18	0·0569
710,000	179 16	0·0240	164 5	0·0342	190,000	191 30	0·0442	187 56	0·0529
700,000	238 3	0·0232	208 26	0·0227	180,000	215 35	0·0395	209 30	0·0477
690,000	292 26	0·0268	269 18	0·0150	170,000	227 38	0·0334	225 8	0·0413
680,000	339 56	0·0312	342 15	0·0141	160,000	238 13	0·0283	236 13	0·0359
670,000	18 56	0·0348	43 10	0·0178	150,000	242 27	0·0254	242 51	0·0332
660,000	57 28	0·0348	93 6	0·0221	140,000	245 45	0·0266	249 6	0·0341
650,000	96 8	0·0310	141 41	0·0227	130,000	254 23	0·0307	259 25	0·0385
640,000	138 28	0·0237	192 38	0·0233	120,000	269 35	0·0356	274 31	0·0426
630,000	195 30	0·0147	247 9	0·0249	110,000	289 28	0·0394	294 7	0·0462
620,000	289 44	0·0127	301 2	0·0287	100,000	312 6	0·0408	316 18	0·0473

Table continued—Perihelion and Eccentricity.

Year.	Stockwell.		LeVerrier.		Year.	Stockwell.		LeVerrier.	
Before 1850.	Long.	Ecc.	Long.	Ecc.	After 1850.	Long.	Ecc.	Long.	Ecc.
90,000	336° 15′	0·0392	340° 3′	0·0452	380,000	299° 29′	0·0232	306° 9′	0·0346
80,000	0 41	0·0343	4 13	0·0398	390,000	336 54	0·0271	336 57	0·0403
70,000	23 36	0·0269	27 22	0·0316	400,000	11 57	0·0296	6 7	0·0429
60,000	40 43	0·0181	46 8	0·0218	410,000	43 58	0·0289	33 10	0·0439
50,000	38 42	0·0110	51 44	0·0129	420,000	71 8	0·0259	58 13	0·0395
40,000	16 31	0·0110	28 36	0·0109	430,000	93 25	0·0200	79 17	0·0341
30,000	20 56	0·0157	25 50	0·0151	440,000	102 25	0·0129	94 0	0·0278
20,000	41 57	0·0192	44 0	0·0188	450,000	77 9	0 0101	98 28	0·0231
10,000	69 28	0·0195	69 47	0·0195	460,000	63 40	0·0172	95 51	0·0235
					470,000	77 23	0·0275	99 5	0·0293
A. D. 1850	100 21	0·0168	100 21	0·0168	480,000	98 48	0·0377	121 2	0·0383
10,000	136 1	0·0115	131 43	0·0149	490,000	124 0	0·0462	133 3	0·0460
20,000	191 53	0·0055	192 13	0·0059	500,000	150 10	0·0518	156 26	0·0523
24,000	----	----	247 18	0·0034	510,000	176 54	0·0536	181 52	0·0553
25,000	----	----	263 25	0·0035	520,000	203 46	0·0510	208 31	0·0543
30,000	305 49	0·0049	318 38	0·0059	530,000	229 47	0·0438	235 46	0·0488
40,000	325 25	0·0077	6 24	0·0124	540,000	253 6	0·0326	262 49	0·0390
50,000	37 22	0·0134	38 3	0·0173	550,000	264 17	0·0247	287 31	0·0259
60,000	64 38	0·0145	64 30	0·0199	560,000	234 6	0·0133	293 9	0·0134
70,000	84 58	0·0134	86 14	0·0200	570,000	186 8	0·0186	229 40	0·0093
80,000	93 40	0·0113	101 41	0·0188	580,000	239 48	0·0376	232 38	0·0221
90,000	88 46	0·0110	109 31	0·0181	590,000	264 17	0·0497	258 21	0·0332
100,000	87 58	0·0143	114 45	0·0192	600,000	293 13	0·0566	288 23	0·0401
110,000	106 26	0·0197	123 30	0·0225	610,000	323 42	0·0577	322 49	0·0439
120,000	117 11	0·0237	137 40	0·0264	620,000	355 2	0·0532	10 54	0·0396
130,000	135 23	0·0261	154 44	0·0299	630,000	27 9	0·0439	40 39	0·0272
140,000	156 54	0·0285	175 35	0·0333	640,000	60 45	0·0311	80 5	0·0230
150,000	176 0	0·0288	192 27	0·0336	650,000	98 42	0·0157	145 19	0·0167
160,000	194 6	0·0280	211 44	0·0336	655,000	126 28	0·0083	----	----
170,000	209 47	0·0264	230 4	0·0325	660,000	204 53	0·0038	217 58	0·0192
180,000	223 31	0·0243	248 21	0·0304	670,000	315 52	0·0142	270 43	0·0265
190,000	233 48	0·0226	265 31	0·0279	680,000	354 26	0·0248	310 37	0·0330
200,000	241 21	0·0219	280 28	0·0248	690,000	29 10	0·0320	345 14	0·0364
210,000	248 39	0·0232	289 46	0·0223	700,000	63 50	0·0349	17 8	0·0357
220,000	260 14	0·0264	297 39	0·0219	710,000	99 50	0·0343	45 43	0·0316
230,000	277 29	0·0300	307 56	0·0235	720,000	138 30	0·0306	74 52	0·0223
240,000	299 19	0·0323	326 47	0·0266	730,000	181 1	0·0257	94 48	0·0105
250,000	334 18	0·0325	350 37	0·0286	737,500	----	----	36 6	0·0033
260,000	351 20	0·0297	19 40	0·0299	740,000	236 30	0·0220	359 44	0·0053
270,000	20 21	0·0238	51 39	0·0275	750,000	295 6	0·0225	0 52	0·0197
280,000	50 11	0·0159	90 10	0·0242	760,000	347 37	0·0262	26 21	0·0335
290,000	79 52	0·0066	128 0	0·0200	770,000	32 18	0 0321	53 47	0·0476
295,000	88 27	0·0018	----	----	780,000	71 53	0·0358	81 26	0·0578
296,000	81 6	0·0009	----	----	790,000	109 25	0·0364	111 29	0·0635
297,000	0 9	0·0003	----	----	800,000	146 44	0·0330	137 4	0·0644
300,000	301 25	0·0028	172 32	0·0162	810,000	189 1	0·0261	169 27	0·0588
310,000	330 0	0·0108	219 47	0·0120	820,000	231 53	0·0148	204 32	0·0506
320,000	4 17	0·0163	244 50	0·0092	830,000	330 17	0·0106	221 14	0·0329
330,000	41 18	0·0188	317 55	0·0039	840,000	52 47	0·0202	228 10	0·0164
335,000	----	----	3 8	0·0012	850,000	99 36	0·0335	176 21	0·0145
337,000	----	----	82 54	0·0007	860,000	138 0	0·0454	177 3	0·0307
337,500	----	----	105 0	0·0009	870,000	173 46	0·0535	201 11	0·0467
340,000	82 35	0·0188	150 5	0·0023	880,000	208 28	0·0568	229 31	0·0589
350,000	131 1	0·0167	202 47	0·0097	890,000	242 44	0·0545	260 2	0·0654
360,000	189 5	0·0158	239 17	0·0184	900,000	277 0	0·0469	291 17	0·0659
370,000	247 35	0·0181	273 45	0·0270	910,000	310 13	0·0350	323 10	0·0608

Table continued—Perihelion and Eccentricity.

Year.	Stockwell.		LeVerrier.		Year.	Stockwell.		LeVerrier.	
After 1850.	Long.	Ecc.	Long.	Ecc.	After 1850.	Long.	Ecc.	Long.	Ecc.
920,000	351° 31′	0·0200	355° 44′	0·0513	1,120,000	322° 21′	0·0355	337° 1′	0·0201
930,000	50 16	0·0038	28 55	0·0376	1,130,000	6 46	0·0429	19 11	0·0342
940,000	154 55	0·0134	65 22	0·0230	1,140,000	46 32	0·0484	55 44	0·0462
950,000	248 33	0·0232	114 16	0·0086	1,150,000	83 26	0·0502	90 29	0·0543
955,000	----	----	184 44	0·0040	1,160,000	118 47	0·0472	124 24	0·0575
956,000	----	----	208 11	0·0034	1,170,000	152 38	0·0392	157 52	0·0554
960,000	282 15	0·0378	267 21	0·0070	1,180,000	185 3	0·0269	191 13	0·0484
970,000	316 11	0·0447	322 11	0·0174	1,190,000	213 22	0·0113	224 49	0·0373
980,000	350 4	0·0478	0 46	0·0255	1,195,000	210 32	0·0032	----	----
990,000	24 25	0·0461	38 32	0·0307	1,196,500	160 32	0·0014	----	----
1,000,000	60 20	0·0414	77 27	0·0334	1,197,500	104 16	0·0021	----	----
1,010,000	99 10	0·0345	118 45	0·0341	1,200,000	86 7	0·0060	259 51	0·0236
1,020,000	144 9	0·0268	161 53	0·0350	1,210,000	107 35	0 0229	301 7	0·0100
1,030,000	199 0	0·0209	206 14	0·0360	1,215,000	----	----	8 19	0·0034
1,040,000	262 6	0·0195	254 13	0·0371	1,220,000	138 6	0·0374	85 15	0·0061
1,050,000	320 33	0·0220	292 29	0·0397	1,230,000	166 56	0·0500	157 52	0·0184
1,060,000	10 11	0·0254	332 48	0·0403	1,240,000	196 24	0·0576	193 14	0·0274
1,070,000	56 18	0·0272	11 43	0·0384	1,250,000	225 21	0·0604	221 49	0·0368
1,080,000	103 14	0·0267	50 13	0·0331	1,260,000	253 36	0·0583	260 54	0·0367
1,090,000	153 50	0·0253	90 18	0·0244					
1,100,000	212 48	0·0248	140 43	0·0132					
1,105,000	----	----	185 14	0·0080					
1,107,500	----	----	222 19	0·0068					
1,110,000	271 9	0·0285	270 16	0·0075					

ART. XV. — *On Crystallized Danburite from Russell, St. Lawrence County, New York;* by GEO. J. BRUSH and EDWARD S. DANA.

Historical Note.—In December last (1879) we received a box of minerals from Mr. C. D. Nims, the well-known mineral collector of Northern New York, containing several specimens labelled "unknown." Among these were a few prismatic white weathered crystals that had been considered to be feldspar, to which our attention was specially called by Mr. Nims. On a pyrognostic examination this substance proved to be an anhydrous boro-silicate, corresponding in physical characters with the rare species *danburite.* Mr. Nims at that time generously placed at our disposal all of the small amount of this material he had in his possession, for scientific examination. These specimens we investigated as thoroughly as they allowed both chemically and crystallographically, and the conclusions reached were identical with those described in this paper. Upon learning further from Mr. Nims that there was a probability of his being able to obtain, in the following spring, more abundant material and of better quality, we deferred publication. Our expectations and those of Mr. Nims have been fully

realized, and the material which he has forwarded to us, as the result of his recent active explorations, is all that could be desired both as to quantity and quality. We take pleasure in acknowledging here our indebtedness to him for his promptness and liberality.

Method of occurrence.—The mineral occurs both crystallized and massive, imbedded in what Mr. Nims calls a granitic rock; the points at which it is found extend along the brow of a hill for a considerable distance, say half a mile. The crystals line cavities or seams, sometimes of very considerable size, in the massive mineral or the enclosing rock. The associated minerals are a pale green pyroxene, a dark brown tourmaline, and also some mica, quartz and pyrite. Of these species, the danburite often encloses the crystals of pyroxene and tourmaline and is itself imbedded in the quartz, which is a point of interest in connection with its time of formation. These cavities were doubtless all filled originally with calcite, as the facts observed conclusively prove. A few perfectly fresh specimens were found with the crystals imbedded in pink calcite and Mr. Nims believes that when the explorations are carried deeper that larger quantities may be obtained. This is much to be desired, for the perfectly clear and transparent crystals found in the calcite are of rare beauty. The specimens here spoken of were actually obtained from some loose bowlders found on the surface.

The most of the specimens are now nearly or quite free from calcite, that mineral evidently having been removed by slow solution. The crystals are thus left in their original position projecting into the cavities. This natural removal of the calcite is in some aspects of the case an advantage, and in others quite the reverse. In no other way could the crystals have been freed from the calcite so perfectly and with so little injury to themselves; for the mechanical removal is out of the question owing to the brittleness of the mineral, and the removal by chemical means in the laboratory would not leave the crystals so nearly in their original condition. On the other hand, the specimens as found are somewhat destitute of freshness of aspect, the crystals being much rifted internally and more or less covered with oxide of iron which cannot be entirely removed. It is to be stated, however, that, while the mineral has thus lost something of its original beauty, it is, in most cases, very little if at all altered chemically, even the luster of the crystalline faces having suffered but little. On some few of the specimens, on the other hand, the crystals are quite opaque and have little luster.

General crystallographic and physical characters.—The danburite from Russell, as has been stated, is in part crystallized

and in part massive. The crystals vary from those which are very minute to others which are of considerable size. The largest isolated crystal has a length of 4 and a width (macrodiagonal) of $2\frac{1}{2}$ inches; some of the groups are really grand in their proportions. The massive mineral can be obtained in large blocks; it shows brilliant luster, is quite unaltered, and almost entirely free from admixed species. The most striking point in regard to the crystals is their similarity to crystals of topaz; so close is this resemblance that the specimens, if not examined too critically, might be handled many times without a suspicion that they did not belong to that species. It will be shown below that this resemblance extends beyond the mere external habit, involving a true homœomorphic relation. The cleavage is basal, as in topaz, but not very distinct.

The hardness of the danburite is 7 to 7·25, and the specific gravity 2·986 to 3·021. The luster on the polished crystalline surfaces is very brilliant; on the fracture and in the massive mineral it is vitreous to greasy; in this form it has much the aspect of common varieties of quartz. The color in the freshest crystals imbedded in calcite is pale wine-yellow, and in others from yellowish-white to honey yellow, dark wine-yellow, and yellowish-brown. The streak is white. The freshest crystals are perfectly transparent, the massive mineral translucent. The fracture is uneven to sub-conchoidal.

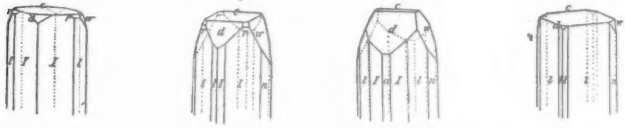
Description of crystalline form.—The crystals are uniformly prismatic in habit. They are commonly attached by one extremity of the prism so that only the other end is terminated; occasional crystals, however, have been observed with terminations alike at both extremities, and hence it is not hemimorphic. The general range of form in the crystals will be gathered from the accompanying figures; figures 1, 2, 3 and 4 show some of the more common and simple forms, and figures 5, 6 and 7 are others more highly modified. It will be noticed that the forms shown in figures 4 and 5 have the appearance of a square prism ($l \wedge l = 94° 52'$). The variety in habit is, as will be seen, very considerable, though the most constantly recurring form is that where the fundamental prism I predominates (fig. 1); the form represented in figure 7 is rather rare.

The crystals belong to the ORTHORHOMBIC SYSTEM; this is proved by the optical examination, since the three axes of elasticity correspond in position to the three crystallographic axes. The measured angles and the general symmetry of the form also correspond to this system.

Notwithstanding the number of the occurring planes the crystals are not often favorable for exact measurement. The planes forming the extremities of the crystals, even of those taken directly from the surrounding calcite, are, with the oc-

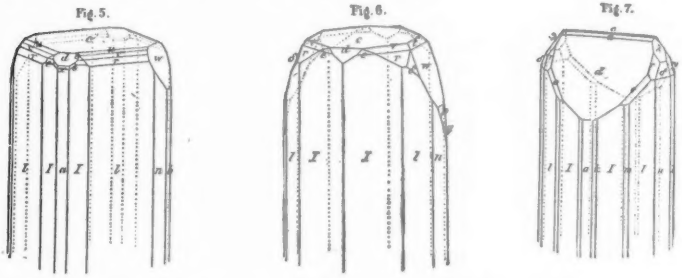
casional exception of the domes d and w, uniformly unpolished and often much rounded. They show, moreover, many partially developed planes which do not admit of determination.

Fig. 1. Fig. 2. Fig. 3 Fig. 4.



In these latter respects they resemble crystals of other species found in a similar situation (e. g. pyroxene in calcite). The determination of the symbols of the various planes was accomplished without difficulty with the aid of the zonal relations

Fig. 5. Fig. 6. Fig. 7.



and with approximate angles, but only very few exact measurements were possible. The prismatic planes I and l, are, however, quite commonly smooth and highly polished. As fundamental angles* the following, as the mean of many single measurements, were accepted:

$$I \wedge I' \quad 110 \wedge \bar{1}10 = 57^\circ\ 7'\ 54''$$
$$d \wedge d' \quad 101 \wedge \bar{1}01 = 82^\circ\ 53'\ 18''$$

From these the following axial ratio is obtained:—

c (vert.)	$\breve{b}$	$\breve{a}$
0·8830	1·8367	1·0000

The observed planes are, as follows:—

c	O	001	s	$3\text{-}\frac{3}{2}$	321
a	$i\text{-}\breve{1}$	100			
b	$i\text{-}\breve{1}$	010	o	1	111
			e	2	221
k	$i\text{-}\frac{3}{2}$	320			
I	I	110	u	$\frac{1}{2}\text{-}\breve{2}$	124
m	$i\text{-}\frac{3}{2}$	230	v	$1\text{-}\breve{2}$	122
l	$i\text{-}\breve{2}$	120	r	$2\text{-}\breve{2}$	121
n	$i\text{-}\breve{4}$	140			

* The angles given are all the supplement angles.

z	$\frac{1}{3}$-$\breve{1}$	103	λ	2-$\breve{4}$	142
d	1-$\breve{1}$	101	δ	4-$\breve{4}$	141
x	3-$\breve{1}$	301			
t	2-$\breve{1}$	021			
w	4-$\breve{1}$	041			
p	8-$\breve{1}$	081			
q	16-$\breve{1}$	0 16 1			

The following is a list of the most important angles for these planes, calculated from the axial ratio given above:—

		on c (001)	on a (100)	on b (010)
k	320	90° 0′	19° 57′	70° 3′
I	110	"	28° 34′	61° 26′
m	230	"	39° 14′	50° 46′
l	120	"	47° 26′	42° 34′
n	140	"	65° 20′	24° 40′
z	103	16° 24′	73° 36′	90° 0′
d	101	41° 27′	48° 33′	"
x	301	69° 19′	20° 41′	"
t	021	43° 52′	90° 0′	46° 8′
w	041	62° 31′	"	27° 29′
p	081	75° 25′	"	14° 35′
q	0161	82° 36′	"	7° 24′
s	321	70° 28′	27° 38′	71° 15′
o	111	45° 9′	51° 29′	70° 11′
e	221	63° 33′	38° 9′	64° 39′
u	124	18° 4′	77° 54′	76° 48′
v	122	33° 8′	68° 18′	66° 16′
r	121	52° 33′	57° 31′	54° 13′
λ	142	46° 37′	72° 21′	48° 40′
δ	141	64° 42′	67° 50′	34° 45′

The angle of the fundamental prism ($I \wedge I'$) as measured on several crystals was 57° 7, 57° 7′, 57° 7, 57° 8, 57° 12′; also for $l \wedge l'$ 94° 53, 94° 52′, 94° 55, 94° 56′ (required 94° 52′). The measured angle $c \wedge w$ (001 $\wedge$ 041) = 62° 33′ (required 62° 31′). The measured angle $I \wedge d = 54° 22′$ (required 54° 27′).

The similarity in general habit between the crystals of danburite and those of topaz has already been remarked. In fact the commonly occurring planes of the danburite crystals are all common planes on crystals of topaz, and of the few planes in the above list which do not belong to topaz all are rare in danburite. A comparison of the angles of the two species shows that the relation in form is really a close one. This will be seen from the following angles in the chief zones.

			Danburite.	Topaz.
$I \wedge I'$	110 $\wedge$ 1$\bar{1}$0	=	57° 8′	55° 43′
$l \wedge l'$	021 $\wedge$ 0$\bar{2}$1	=	94° 52′	93° 11′
$d \wedge d'$	101 $\wedge$ $\bar{1}$01	=	82° 53′	83° 54′
$w \wedge w'$	041 $\wedge$ 0$\bar{4}$1	=	125° 2′	124° 40′
$c \wedge o$	001 $\wedge$ 111	=	45° 9′	45° 35′
$c \wedge e$	001 $\wedge$ 221	=	63° 33′	63° 54′

The axial ratios for the two species are:—

	c (vert.)	$\breve{b}$	$\breve{a}$
Danburite	0·8830	1·8367	1·0000
Topaz	0·9024	1·8920	1·0000

The above values show that the two species are closely homœomorphous.

Optical properties.—The transparent crystals of danburite offered very good material for the determination of the optical properties of the species, and they prove to be of rather unusual interest. The first point established was the position of the axes of elasticity, which were found to coincide, as stated above, with the crystallographic axes. The optic axes lie in the basal plane, and the axial angles are so large that it was necessary to measure both of them in oil. By this measurement the interesting result was reached that the acute bisectrix for the lower end of the spectrum (red and yellow rays) is normal to the brachypinacoid, and for the upper end of the spectrum (blue rays) normal to the macropinacoid.

From a section cut parallel to the brachypinacoid the following angles were obtained, each being the mean of a large number of measurements:

Red (Lithia flame).	Yellow (Sodium flame).	Blue ($CuSO_4$ solution).
100° 33′	101° 30′	104° 36′

From a section parallel to the macropinacoid the angles obtained in the same manner were:

Red (Li).	Yellow (Na).	Blue ($CuSO_4$).
106° 35′	105° 36′	102° 13′

From these angles the true internal axial angle was calculated by the usual method; the results are:

	Bisectrix normal to b (010).	Bisectrix normal to a (100).
Red (Li)	87° 37′	92° 23′
Yellow (Na)	88° 23′	91° 37′
Blue ($CuSO_4$)	90° 56′	89° 4′

The bisectrix normal to the brachypinacoid is negative, and that normal to the macropinacoid is positive.

The index of refraction of the oil employed was found to be for

Red (Li) = 1·4706; Yellow (Na) = 1·4735; Blue ($CuSO_4$) = 1·483

For obvious reasons the last value is less accurate than the other two. Making use of these values in the usual formulas, the mean index of refraction (β) for danburite is obtained, viz:

$\beta = 1{\cdot}634$, Red (Li)
$= 1{\cdot}637$, Yellow (Na)
$= 1{\cdot}646$, Blue ($CuSO_4$)

It is obvious from the values of the axial angles for the different colors given above, that for certain rays, those falling in

the lower end of the blue, the axial angle must be for the ordinary temperature *exactly* 90°. It would be easy to calculate the wave-length of the rays answering this condition, but since the sections employed were not faultless the angles are not very accurate and hence the calculation would have but little value.

Optically danburite does not agree very closely with topaz, for with the latter species the axes lie in the brachydiagonal, the vertical axis coinciding with the acute bisectrix; the axial angle is also quite different. It is interesting to note, however, that the mean indices of refraction are not far apart; thus for the D line in the spectrum, we have

β, Danburite = 1·637
Topaz = 1·6138

Chemical composition.—The quantitative chemical examination of the mineral was made by Mr. W. J. Comstock, of the Sheffield Laboratory, to whom we wish here to express our grateful acknowledgments for the following analyses:

	I.	II.	III.	IV.	Mean.
Silica	48·16	48·30	----	----	48·23
Boron trioxide	----	----	26·67	27·18	26·93
Lime	23·26	23·22	----	----	23·24
Alumina*	0·48	0·46	----	----	·47
Ignition	0·64	0·63	----	----	·63
					99·50

* With trace Fe_2O_3.

The mineral was decomposed by fusion with sodium carbonate for the silica and bases in Nos. I and II, and the boric acid in Nos. III and IV was obtained by Stromeyer's method as potassium boro-fluoride. A further decomposition was effected with fluohydric acid to make special examination for alkalies, which gave a negative result.

In view of the close homœomorphism of our mineral with topaz, we requested Mr. Comstock to make special examination for fluorine, but the result proved the absence of this and allied elements.

Mr. Comstock's analyses offer a remarkable confirmation of the analyses of Smith and Brush* of the Danbury mineral, the mean of which gave

SiO_2	B_2O_3	Al_2O_3, Fe_2O_3	Mn_2O_3	CaO	MgO	ign.
48·15	27·15	0·30	0·56	22·37	0·40	0·50 = 99·43

The quantivalents ratio from the mean of Comstock's analyses gives for $SiO_2 : B_2O_3 : CaO = 8·04 :: 3·88 :: 4·14$ and that from the analyses of Smith and Brush is $8·02 :: 3·89 :: 4·10$. There can be no question that the true theoretical ratio is 2 : 1 : 1. This leads to the formula previously accepted, that

* This Journal, II, xvi, 365, 1853.

is, $CaB_2Si_2O_8$, or $Ca_2SiO_4+B_4Si_2O_{12}$. The formula requires: Silica, 48·78, boron trioxide, 28·46, lime, 22·76=100. These results set at rest any further question as to the chemical composition of danburite. It does not appear, however, that there is any immediate relation between danburite and topaz in chemical composition, which, considering the similarity in crystalline form, is rather remarkable.

Pyrognostic characters.—The pyrognostic characters of this species are sufficiently important to be here repeated. B. B. the mineral glows, fuses gently at 3·5 to a colorless glass, imparting to the flame the characteristic green color due to boron. On cooling, the assay loses its transparency and becomes milk-white. In the closed tube it phosphoresces brilliantly with a reddish yellow light. The mineral is slightly acted upon by hydrochloric acid, sufficiently so to give the reaction for boric acid with turmeric paper. When previously ignited to the point of fusion the mineral gelatinizes with acid.

Comparison with the original danburite.—A comparison between the characters of the danburite from Russell, N. Y., and those of the same species from the original locality at Danbury, Conn., shows a very close agreement in all essential respects, which removes all doubt as to the real identity of the mineral now described. In crystalline form alone is there apparent divergence. In regard to this there is only to be said that the earlier determinations upon the Danbury mineral were made on imbedded fragments in feldspar where apparent planes, at best of a problematical nature, certainly did not represent the true crystalline form of the species.

ART. XVI.—*On a Photograph of Jupiter's Spectrum, showing Evidence of Intrinsic Light from that Planet;* by Professor HENRY DRAPER, M.D.

[Read before the Royal Astronomical Society, May 14th, 1880, and extracted from the Monthly Notices.]

THERE has been for some years a discussion as to whether the planet Jupiter shone to any perceptible extent by his own intrinsic light, or whether the illumination was altogether derived from the sun. Some facts seem to point to the conclusion that it is not improbable that Jupiter is still hot enough to give out light, though perhaps only in a periodic or eruptive manner.

It is obvious that spectroscopic investigation may be usefully employed in the examination of this question and I have incidentally, in the progress of an allied inquiry,* made a photo-

* See paper "On Photographing the Spectra of the Stars and Planets," read before the National Academy of Sciences, Oct. 28, 1879, and published in this Journal, Dec., 1879, and in Nature, Nov. 27, 1879.

graph which has sufficient interest to be submitted to the inspection of the Astronomical Society.

If the light of Jupiter be in large part the result of his own incandescence, it is certain that the spectrum must differ from that of the sun, unless the improbable hypothesis be advanced that the same elements, in the same proportions and under the same physical conditions, are present in both bodies. Most of the photographs I have made of the spectrum of Jupiter, answer this question decidedly, and from their close resemblance to the spectrum of the sun indicate that, under the average circumstances of observation, almost all the light coming to the earth from Jupiter must be merely reflected light originating in the sun. For this reason I have used the spectrum of Jupiter as a reference spectrum on many of my stellar spectrum photographs.

But on one occasion, viz: on September 27, 1879, a spectrum of Jupiter with a comparison spectrum of the moon was obtained which shows a different state of things. Fortunately, owing to the assiduous assistance of my wife, I have a good record of the circumstances under which this photograph was taken, and this will make it possible to connect the aspect of Jupiter at the time, with the spectrum photograph, though I did not examine Jupiter with any care through the telescope that night, and indeed did not have my attention attracted to this photograph till some time afterwards.

I send herewith to the Astronomical Society for examination, the original negative which is just as it was produced, except that it has been cemented with Canada balsam to another piece of glass for protection. Attached to the photograph is an explanatory diagram, intended to point out the peculiarities which are of interest. It will be noticed at once that the main difference is not due to a change in the number or arrangement of the Fraunhofer lines, but rather to a variation in the strength of the background. In the case of the moon the background is uniform across the width of the spectrum in any region, but in the case of Jupiter the background is fainter in the middle of the width of the spectrum in the region above the line *h*, and stronger in the middle in the region below *h*, especially toward F. The observer must not be confused by the dark portion where the two spectra overlap along the middle of the combined photograph.

In order to interpret this photograph it must be understood that the spectrum of Jupiter was produced from an image of the planet thrown upon the slit of the spectroscope, by a telescope of 183 inches focal length, the slit being placed approximately in the direction of a line joining the poles of the planet. The spectroscope did not, therefore, integrate the light of the whole

disk, but analyzed a band at right angles to the equator and extending across the disk. If either absorption or production of light were taking place on that portion of Jupiter's surface there might be a modification in the intensity of the general background of the photographed spectrum.

A casual inspection will satisfy any one that such modifications in the intensity of the background are readily perceptible in the original negative. They seem to me to point out two things that are occurring: first, an absorption of solar light in the equatorial regions of the planet; and second, a production of intrinsic light at the same place. We can reconcile these apparently opposing statements by the hypothesis that the temperature of the incandescent substances producing light at the equatorial regions of Jupiter did not suffice for the emission of the more refrangible rays, and that there were present materials which absorbed those rays from the sunlight falling on the planet.

If the spectrum photograph exhibited only the absorption phenomenon above *h*, the interest attached to it would not be great because a physicist will readily admit from theoretical considerations that such might be the case owing to the colored belts of the planet. But the strengthening of the spectrum between *h* and F in the portions answering to the vicinity of the equatorial regions of Jupiter bears so directly on the problem of the physical condition of the planet as to incandescence that its importance cannot be overrated.

The circumstances under which this photograph was taken were as follows: Longitude of observatory $4^h\ 55^m\ 29^s{\cdot}7$ west of Greenwich. Night not very steady. Jupiter and the moon differed but little in altitude. Jupiter's spectrum was exposed to the photographic plate for fifty minutes, the moon was exposed for ten minutes. Jupiter was near the meridian. The photograph of Jupiter's spectrum was taken between $9^h\ 55^m$ and $10^h\ 45^m$, New York mean time, September 27, 1879.

I have suspected that perhaps there may have been an influence produced by the great colored patch on Jupiter which has made itself felt in this photograph. It may be that eruptions of heated gases and vapors of various composition, color, and intensity of incandescence are taking place on the great planet, and a spot which would not be especially conspicuous from its tint to the eye might readily modify the spectrum in the manner spoken of above.

ART. XVII.—*On the Spectrum of the Flame of Hydrogen;* by WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received June 16, 1880.

MESSRS. Liveing and Dewar state, in a paper read before the Royal Society on June 10, that they have obtained a photograph of the ultra-violet part of the spectrum of coal gas burning in oxygen, and in a note dated June 8th they add that they have reason to believe that this remarkable spectrum is not due to any carbon compound but to water.

Under these circumstances I think it is desirable that I should give an account of some experiments which I made on this subject some months since, without waiting until the investigation is more complete.

On December 27, 1879, I took a photograph of the flame of hydrogen burning in air. As is well known, the flame of hydrogen possesses but little luminosity, and shows no lines or bands in the visible part of the spectrum, except that due to sodium as an impurity.

Professor Stokes, in his paper "On the Change of Refrangibility of Light,"* has stated that "the flame of hydrogen produces a very strong effect. The invisible rays in which it so much abounds, taken as a whole, appear to be even more refrangible than those which come from the flame of a spirit lamp." I was not, however, prepared for the strong group of lines in the ultra-violet which, after an exposure of one minute and a half, came out upon the plate.

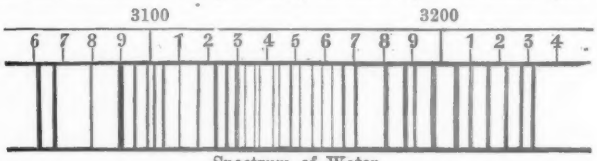
Two or three weeks later, about the middle of January, 1880, I showed this spectrum to Professor Stokes, and we considered it probable that this remarkable group was the spectrum of water. Professor Stokes permits me to mention that, in a letter addressed to me on January 30, he speaks of "this novel and interesting result," and makes some suggestions as to the disputed question of the carbon spectrum.

I have since that date taken a large number of photographs of the spectra of different flames, in the hope of being able to present the results to the Royal Society, when the research was more complete. I think now that it is desirable that I should describe the spectrum of the flame of hydrogen, but I shall reserve for the present the experiments which relate to the presence of carbon and its compounds.

The spectrum of the flame of hydrogen burning in air (No. 1) consists of a group of lines which terminates at the more refrangible limit in a pair of strong lines, λ 3062 and λ 3068. At a short distance, in the less refrangible direction, what may

* Phil. Trans., 1852, p. 539.

perhaps be regarded as the group proper, commences with a strong line, λ 3090. Between the strong line λ 3068 and the line λ 3090 there is a line less bright, λ 3080. Less refrangible than the line λ 3090 are finer lines at about equal distances. The lines are then fine and near each other, and appear to be arranged in very close pairs. There is a pair of fine, but very distinct lines, λ 3171 and λ 3167. In this photograph the group can be traced to about λ 3290. This group constitutes the whole spectrum, which is due probably to the vapor of water (see fig.)



Spectrum of Water.

I then introduced oxygen into the flame, leaving a small excess of hydrogen. A spectrum in all respects similar came out upon the plate. I repeated the experiment, taking both spectra on the same plate. Through one-half of the slit the spectrum of the oxyhydrogen flame was taken. This flame was about seven inches long, and the spectrum taken of a part of the flame two inches from the jet. The oxygen was then turned off, and the quantity of hydrogen allowed to remain unaltered. A second spectrum with an exposure of the same duration was then taken through the second half of the slit. On the plate the two spectra are in every respect similar, and have so exactly the same intensity, that they appear as one broad spectrum.

In all these experiments a platinum jet which had been carefully cleaned was used.

In these experiments the two gases met within the blowpipe and issued in a mixed state.

The jet was removed, and a flame of hydrogen was surrounded with oxygen. The spectrum (No. 2) shows some additional lines, In this case the jet was brass, and in this or some other way impurities may have been introduced; and I should, at present, incline to the view that the additional lines about λ 3429 and λ 3473, and the groups more refrangible than λ 3062, do not belong to the water spectrum, but to impurities.

Coal-gas was substituted for hydrogen in the oxyhydrogen blowpipe, and oxygen admitted in as large a proportion as possible. The inner blue flame rising about two inches above the jet showed in the visible part of the spectrum the usual "five-fingered spectrum." The light from this part of the flame was projected upon the slit. The spectrum, (No. 3), contains the water group already described, and in addition a very strong line close to G, and two lines, λ 3872 and λ 3890; this latter

line is seen to be the more refrangible limit of a group of fine lines shading off towards K.

The ultra-violet group when carefully compared with the group in the spectrum of pure hydrogen, shows several small differences. I am inclined to believe that there is the superposition of a second fainter group. There is strong evidence of this in some spectra of hydrogen taken under other conditions. There is also a broad band less refrangible than the strong line at G, and the light extends from this line on its more refrangible side.

A double Bunsen burner (Fletcher's form) with a strong blast of air was then fitted up. The spectrum was taken of the intense blue flame. It resembles the one last described. All the distinctive features are intensified and a continuous spectrum and groupings of very fine lines fill up all the intervals between the groups already described, so that there is an unbroken strong spectrum throughout the whole region which falls upon the plate.

A spirit lamp was arranged before the slit. The spectrum is essentially the same as No. 3, but as it is less intense only the strongest lines are seen. The water group, the strong line at G, and the pair of lines rather more refrangible than K, are seen. Probably with a longer exposure the finer lines would also show themselves.

The distinctive features of spectrum No. 3 appear to be connected with the presence of carbon.

Table of Wave-lengths of the Principal Lines of the Spectrum of Water.—No. 1.

3062	3090	3117	3142·5	3167	3198	3232
3068	3094	3122·5	3145	3171	3201	3242·5
3073	3095	3127	3149·5	3175	3207·5	3252.5
3074	3099	3130	3152·5	3180	3211	3256
3077·5	3102	3133	3156	3184	3217·5	3262
3080	3105	3135	3159·5	3189	3223	3266
3082	3111	3139	3163	3192·5	3228	3276
3085						

Wave-lengths of other Lines in Spectrum.—No. 2.

2869·5	2897	2929	2959	2994	3029	3271
2872.5	2904	2932·5	2966	2999	3031	3429·5
2876	2907·5	2935·5	2967·5	3002	3039	3473
2880	2910	2940	2970·5	3005	3042	
2883	2913	2943	2975·5	3010	3046	
2887·5	2917·5	2947	2981	3013	3051	
2892	2922·5	2951	2989	3017	3057·5	
2895	2925·5	2955	2991	3019·5	3246	

Wave-lengths of other Lines in Spectrum.—No. 3.

3872	3890	4310

ART. XVIII.—*Determination of the Acceleration due to the Force of Gravity, at Tokio, Japan;* by T. C. MENDENHALL.

THE following series of determinations of the value of the acceleration due to the force of gravity at this place was begun in the month of February of the present year and continued throughout the succeeding two or three months. A considerable time was devoted to preliminary experiments, and there was some delay on account of the non-arrival of one of the pendulums used, from the maker in Europe. Throughout the whole series and in all of the labor connected with it I have had the intelligent and faithful assistance of Messrs. Tanaka and Tanakadate, two special students in the department of Physics of the Imperial University of Japan.

There is probably nothing new in the method employed, except, perhaps, the mode of determining the time of vibration of the pendulum. As far as I am aware this method has not been previously described, and it seems to possess many advantages over the ordinary method of coincidences. It simply involves the use of a good chronograph and a break-circuit clock or chronometer, together with an arrangement by means of which the experimental pendulum can be made to record its own beats upon the chronograph at any time. In the beginning the whole number of vibrations which the pendulum will make in a given time may be determined by letting it break the circuit at every vibration, or, better, at every sixtieth or hundredth vibration, which can easily be accomplished by counting and raising the break-circuit apparatus to its proper position underneath the pendulum at the right moment. In our arrangement this apparatus consisted of a very small and light "trip-hammer" made of fine wire, which was so adjusted that by pressing upon a button it was brought up to such a point that it would be just "thrown" by the pendulum in its passage through the lowest point of its arc. Although the resistance offered to the pendulum can be made extremely small, yet it is so great as to interfere quite perceptibly with its motion if the pendulum is obliged to operate the break-circuit at each beat, as experiment has proved. But it may be rejected after the first two or three trials, not only on account of the resistance which it introduces but also because it is not necessary to continue its use. The whole number of seconds required for a given number of vibrations being known, it only remains to determine the fractional part of a second as accurately as possible. It is therefore only necessary to cause the pendulum to break the circuit twice, once at the beginning of the period and once again at the end. By this means all

objection to the process on account of resistance is removed. Indeed it is in the possibility of determining these fractional parts of a second at the beginning and at the end, that the merit of this method consists. The chronograph used in these determinations is by Alvan Clark and Sons, and for uniformity of speed it is everything that could be desired. The line made by the pen is sharp and clear. The length of one second on the sheet is about 8 mm., so that it can be easily measured with a microscope of low power with a micrometer eye piece. It will easily be seen that even if the total time during which the pendulum is made to swing be not great, its value can be ascertained within a very small fraction of itself. By this process, therefore, it becomes possible to make the duration of the experiment extremely short compared with that required in the method of coincidences and yet to reach the same degree of accuracy. As a proof of this it may be stated that in numerous instances in which the duration of the experiment was only twenty minutes, three independent measurements of the total time, made from the chronograph sheet, did not differ among themselves by more than one sixty-thousandth part of the whole. The advantages in thus reducing the whole duration of the experiment from hours to minutes are many. All of the conditions may be maintained nearly constant during the whole time of the swing, and this is especially important in regard to temperature and arc of vibration, the latter being also made much smaller to begin with than would otherwise be possible. Again, the method eliminates "judgment" to a great extent as the pendulum marks for itself the beginning and the end of the period of time. Another important gain is that the use of the clock may be dispensed with, and, without loss of accuracy, the break-circuit chronometer substituted, thus rendering the whole apparatus for such a determination easily portable.

The first series of experiments undertaken was with a Kater's pendulum. The adjustment of the sliding weights, for the purpose of making the times of vibration when suspended from either knife edges equal to each other, is a matter of considerable difficulty and involves much labor. In the present instance no attempt was made to secure a closer agreement between the two periods than that of those recorded below, for the reason that the difficulties in the way of obtaining the exact length of the pendulum were such as to make any greater degree of accuracy as far as time is concerned unnecessary. There being at that time no standard of length in the possession of the University, recourse was had to a standard bar three meters in length, made by Troughton, belonging to the Department of Public works. As the pendulum was approximately

one meter long it was necessary to "triplicate" it in order to compare it with this bar. The latter is provided with reading microscopes, and by repeating the comparison two or three times the result given below was obtained and it is believed to be not more than a few hundredths of a millimeter from the truth. The results of six determinations of the time of vibration are given below in columns A and B, the first set being made upon one of the knife edges, and the second upon the other. The values of "g" corresponding to these are also given.

Length of pendulum at 0° 1·00069 meters.
" " at 11°·5 (vibrating temp.) 1·00090

Time of vibration, corrected for arc and chronometer.

A.	B.
1·00410	1·00412
1·00412	1·00417
1·00410	1·00417
Values "g."	
9·7980	9·7976
9·7976	9·7966
9·7980	9·7966

These give a mean value of 9·7974.

It must be remembered, however, that these results are not corrected for buoyancy, in other words they are still to be reduced to a vacuum. This correction has not been applied for two reasons: first, because the result cannot be considered as anything more than a first approximation, and, second, because I am by no means certain as to just what it ought to be. There is no doubt but the correction must be different in the two cases of suspension, when the heavy ball of the pendulum is above the knife edge and when it is below, as was experimentally demonstrated by Sabine many years ago. I believe Sabine also investigated the actual correction to be applied to Kater's pendulum, but owing to the very limited library facilities afforded residents of this country I am unable to refer to his results. When corrected, the above mean will undoubtedly somewhat exceed the true value, but no great degree of accuracy was attempted either in the adjustment of the pendulum or in the measurement of its length.

A "Borda's Pendulum," made by Salleron, was received soon after the conclusion of the above experiments, and as it was accompanied by a standard measuring apparatus it was determined to undertake a more accurate and a more extended series of observations for the determination of the value of "g."

The pendulum was of the well known form used by Borda and by many others since his time. The ball was of brass,

and upon examination was found to be very closely spherical and homogeneous. It could be suspended by means of a small concave cup to which the ball would cling when it was rubbed with a little tallow. To this cup a suspending wire was fastened, the other end of which was secured to the end of a small cylinder projecting down from the knife edge. In a line with this and above the knife edge was a set of adjusting screws, by means of which this part of the apparatus, independent of the wire and ball, was made to vibrate in approximately the same period as that of the pendulum. Experiment proved that this adjustment might be very considerably disturbed without sensibly affecting the time of vibration of the pendulum, but it was, nevertheless, carefully attended to. After a number of trials the cup was rejected as a useless and somewhat uncertain part of the apparatus, and in its stead, in the final series of experiments, the wire was securely fastened to the ball by means of a small drop of solder which was fused on the end of the wire and afterward brought in contact with the ball while the latter was heated. The rejection of the cup greatly simplifies the calculation of the reduced length of the pendulum and lessens the probability of error in the measurements. A number of trials were made to determine the most desirable suspending wire. Theoretically an exceedingly fine wire is best, but practically it was found that much trouble was occasioned by the almost constant stretching and final breaking of very fine wires, so that it was found best to employ a wire of sufficient strength to assure, after a few days' suspension, a pendulum of invariable length during the time of the experiment, subject, of course, to slight changes due to variable temperature. The wire used at last was of platinum, which is desirable on account of its low coefficient of expansion. The measuring apparatus was similar to that used by Borda. The great advantage of this method is that the pendulum can be measured *in place*, or at least it must only be removed to give place to the measuring rod, the actual adjustment for length having been made before. This pendulum, as in the previous case, was suspended from a strong iron support secured to the top of a stone pier about six feet in height and two feet square. The knife edge of the pendulum rested on a pair of agate plates which were accurately leveled by means of four leveling screws and then firmly secured, so that motion was impossible. At the base of the stone pier and firmly secured to it was a strong wooden beam, upon which could be placed the apparatus for breaking the circuit, and also the small circular plane table which was elevated by means of a screw until it was tangent to the sphere at its lowest point. The measuring rod which could be substituted for the pendulum was of iron, and the length of the rod was read by means

of a vernier and microscope to hundredths of a millimeter. There was also a metallic thermometer attached for giving the temperature of the bar. The reading of the length of the rod was always taken while the rod was in position upon the agate plates, and its temperature was read at the same time. The deflection of the support of the pendulum due to the addition of the weight of the measuring rod was determined to be about ·02 mm. and correction was made for it. As the apparatus was arranged it was very easy to make a measurement of the length of the pendulum, and measures were taken at very short intervals, always both before and after a series of vibrations. It was found, however, that when the temperature was constant the length remains sensibly the same. A number of comparisons were made between the meter upon the measuring rod and a standard meter from the Finance Department, made by Deleuil, both of which ought to be correct at 0°. The result of these comparisons was such that a correction of −·04 mm. was made upon the length of the measuring rod at 0°. The pendulum was suspended in a small room favorably situated as regards all disturbances from air currents and sudden changes in temperature. From this room wires were carried to the chronograph, which was in a room near by. The time was taken from a break-circuit chronometer in the transit room of the Astronomical Observatory. The chronometer was not moved from its place, the observatory being connected with the physical laboratory, from which it is distant nearly two miles, by a telegraph line, so that the beats of the chronometer were recorded upon the chronograph in the laboratory. The rate of the chronometer was determined by star transits observed for several nights in succession before and after the date of the observations recorded below. The results given are the periods of vibration in mean solar time, corrected for chronometer rate and also for arc of vibration, the latter being observed by means of a scale and a telescope about fifteen feet away. The mean arcs varied, in the different experiments, from 40′ to 70′.

After having found the total length of the pendulum, as well as the dimensions and masses of its various parts, the "reduced length," or the length of the equivalent simple pendulum, is computed by means of a well-known formula. This with the time of vibration gives the value of "g" in air, and to this must be added the correction for "buoyancy." In most of the earlier determinations by this method this correction was found by simply comparing the density of the pendulum with that of the air in which it vibrated. Although it was shown, before Borda made his experiments, that this correction was too small, he seems to have been ignorant of the fact, and not

until Bessel again investigated the question a good many years later, was general attention called to the fact. About the year 1830, Baily made an extensive series of experiments with a variety of pendulums, for the purpose of determining the true value of this correction. Unfortunately I am unable to refer to his results directly, but I believe that his conclusion was that for such a pendulum as was used in these determinations the ordinary correction should be multiplied by 1·5, and this factor has been used in correcting the results given.

Below will be found the results of eleven different determinations, the first five of which were made on May 26, and the others on May 27. On both days during the time of vibration, all of the conditions were sensibly constant and the same, and in addition to this the nights were favorable for the determination of the chronometer rate. Each of the results is based upon an experiment of twenty minutes' duration, the time of vibration in each case being the mean of two or three independent measurements of the chronograph record made by different persons.

The value of "g" is calculated for each time of vibration determined, and each is corrected for buoyancy. These include all of the determinations made upon those two days, none having been rejected.

Determination of the value of "g" at Tokio, Japan.

Latitude N. 35° 41′, long. E. 139° 44′. Ht. above sea level, 5 meters.

Total length of pendulum,	1014·18 mm.
Distance from knife edge to wire,	46·50 "
Length of wire,	931·62 "
Radius of ball,	18·03 "
Weight of ball,	198·951 grm.
" wire,	1·913 "
Density of ball,	8·0

Length of equivalent simple pendulum, 994·59 mm.

Time of vibration.	Corresponding value of "g."
1·00103	9·7982 meters.
1·00100	9·7988 "
1·00103	9·7982 "
1·00104	9·7980 "
1·00103	9·7982 "
1·00101	9·7986 "
1·00102	9·7984 "
1·00101	9·7986 "
1·00103	9·7982 "
1·00101	9.7986 "
1·00100	9·7988 "

Mean of all results, g=9·7984.

This result is slightly greater than that given by many of the formulas for computing the value of "g" in any latitude.

An excellent opportunity is offered in Japan for measuring the force of gravity at a considerable height above the sea level, in the great extinct volcano, Fujiyama, which reaches a height of between 12,000 and 13,000 feet. An excursion is being arranged for the purpose of making this determination during the coming summer. For this purpose what may be called an "invariable" pendulum is now being vibrated in the place at which the above result was obtained. Its period will be carefully ascertained here before carrying it to the mountain, then on the top of the mountain, and again here after it has been brought back.

Nearly all of the labor of the above determination had been concluded when the April number of the Philosophical Magazine reached Japan. It was found to contain a paper by Messrs. Ayrton and Perry, on a "Determination of the Acceleration of Gravity for Tokio, Japan," which was based on experiments made by the authors, at the College of Engineering in this city, in 1878. Before beginning the series of experiments described above, I had learned that some work in the same direction had been done in the College of Engineering. Messrs. Ayrton and Perry being no longer in Japan, I was unable to ascertain, although inquiry was made, anything very definite concerning their methods or results. I was led to believe, however, that they had made no attempt at a very precise determination. Their paper, as now published, puts the matter in a somewhat different light, and, as their result differs from that deduced above, I desire to call attention to some points in connection with their method and their calculation. The data furnished in the paper are by no means as complete as would be desirable for a thorough discussion of its value, but much may be learned by an examination of what it does contain.

The pendulum used by Messrs. Ayrton and Perry was nearly ten meters in length. There are serious objections to the use of a long pendulum. Borda, in his celebrated determinations made at Paris, used a pendulum about four meters long, but one which approximates in length to a seconds pendulum has been almost universally made use of since. The great objection to the use of a long pendulum is the difficulty of measuring it *in place*. Messrs. Ayrton and Perry measured their pendulum by placing it in a horizontal position, and stretching it by allowing the end near the ball to hang over a wheel with very little friction. The length was obtained by comparison with a bar one meter long, and as this bar must be

placed ten times to cover the whole length, it is plain that any great degree of accuracy must have been difficult to obtain, and this is especially true when the measurement of that portion of the wire which hangs over the wheel is considered. Their 26th experiment was made on the 25th of January, and the 53d on the 21st of February, from which we may infer that the entire time of suspension was at least two months. As only one measurement is spoken of, it is probable that it was measured at the conclusion of the series of experiments, and it seems hardly likely that its length would have remained constant during that length of time. In getting the time of vibration the first method used was what might be termed the method of coincidences by electricity, and which, so far as I know, was first described by Professor Pickering, in his excellent "Physical Manipulations." This was afterward rejected, however, and the vibrations were counted by means of a Morse instrument. The authors speak of measuring the fraction of a vibration, but evidently this could not be done with accuracy by the use of such an arrangement, and there is also the objection that the pendulum was obliged to do the work of breaking the circuit at every vibration. Messrs. Ayrton and Perry give the time of vibration of their pendulum for only three experiments, and it is a little difficult to understand exactly how these were obtained. The time, taken from the chronometer, is given and also the number of vibrations. Any one who will take the trouble to divide one by the other, will obtain results differing very materially from those given in the paper. The only way out of this difficulty that occurs to me, is to consider the times given as the apparent times corrected for clock error, and this I shall do, although it involves the somewhat violent assumption that the chronometer employed had, in the first experiment, a "losing rate" of about 1 minute and 15 seconds per day, which, at the time of the second experiment had changed to a slight "gaining rate," and had fallen again to a "losing rate" at the third. Supposing, however, that this was the case, it will be observed that the periodic time used in the calculation is considerably greater than that of either of the three experiments given. This time, 3·0748 seconds, is doubtless the mean time, and in the absence of the other results it may be fairly assumed that there was at least one result which was as much greater than the mean as the least of those quoted in the paper is less. On this supposition, the extreme times of vibration of their pendulum will be 3·0741 seconds and 3·0755 seconds. Substituting these numbers in their formula, the values of "g" obtained would be, respectively, 9·7997 meters and 9·7907 meters. These results are not corrected for buoyancy, but the same wide range would exist

after that correction is made. But as nothing is known concerning the experiments not quoted, the most favorable case imaginable may be assumed. Suppose that in addition to the three results given, they were in possession of an infinite number of others, all agreeing exactly with that which is used in their computation. The extreme results would still cover a wide range, from 9·7997 to 9·7952. Instead of the latter number they give 9·7958, but they have made a miscalculation in reducing the formula, the true value being as given above.

Although making some elaborate calculations concerning corrections which may be rejected, they reject, apparently without calculation, the correction for the arc of vibration. The arc through which their pendulum swung was nearly 2°, and if a correction for this be applied it will materially alter the last figure of their result. In applying the correction for buoyancy they seem to have corrected only for the ordinary density of the ball and not for its "vibrating density." The correction which they apply is ·0016 meters, and if this be multiplied by 1·5 and the correction for arc also made, their result will be 9·7979 instead of 9·7974, as given in their paper. They refer to the close agreement of their result with that deduced from Clairault's formula, which they make to be 9·797. But they have misquoted Clairault's formula, putting γ where there should be 2γ. This is doubtless a typographical error, but they have also miscalculated it, for it gives 9·7980 meters as the value of "g" at this latitude, and not 9·797 meters, as stated in their paper.

Taking all of these facts into consideration, it does not seem that great weight can be attached to their result, notwithstanding its close agreement with the calculated value for this place, and it is doubtful whether the "bounds of existing knowledge" have been advanced, in any great degree, by this investigation.

Tokio, Japan, June 2, 1880.*

ART. XIX.—*On a new species of Ptericthys, allied to Bothriolepis ornata* Eichwald, *from the Devonian rocks of the North side of the Baie des Chaleurs;* by J. F. WHITEAVES.

THE nomenclature of some of the Devonian Placoderms of the sub-order Ostracostei of Huxley is still in a state of great confusion. Thus, *Ptericthys* Agassiz and *Bothriolepis* Eichwald, are both quoted by Pander as synonyms of *Asterolepis* Eichwald, while the *Asterolepis* of Agassiz and Hugh Miller is regarded by the same authority as synonymous in part with

* Received at New Haven, Ct., June 29, 1880.

Homostius Asmuss, and in part with *Heterostius*. On the other hand, Prof. R. Owen claims* that *Pterichthys* should be retained in preference to *Asterolepis* and *Bothriolepis* Eichwald, on the ground that "no recognizable generic characters were associated" with the latter names; and, as this view has been very generally accepted by paleontologists, it will be adopted provisionally in these notes.

The only remains of fossil fishes yet recorded as occurring in the Paleozoic rocks of North America which may prove to be referable to the genus *Pterichthys*, are some isolated scales from the Catskill group of Tioga County, Pennsylvania, described by Prof. Hall in 1843 as *Sauripteris Taylori*, but which Dr. Newberry thinks have the characteristic sculpture of *Bothriolepis*. The name *Pterichthys Norwoodensis*, although inadvertently cited by Mr. S. A. Miller, on page 238 of his "American Palæozoic Fossils," should have been rejected long ago, for in the first volume of the Second Series of this Journal, dated 1846, Drs. Norwood and Owen showed that the specimen for which it was suggested is the type of their genus *Macropetalichthys*, and of a species which they described as *M. rapheidolabis*.

In the summer of 1879, Mr. R. W. Ells, M.A., of the Geological Survey of Canada, had the good fortune to find, in a concretionary nodule of argillite from the north side of the Baie des Chaleurs immediately opposite Dalhousie, a mould of the plastron or ventral surface of a true *Pterichthys* (as defined by Prof. Owen) with one of the pectoral spines in situ. At the earliest practicable opportunity, Mr. Ells revisited the locality, and in the first week of June last obtained three exquisitely preserved specimens of the buckler of the same species and several fragments; also some isolated scales of a *Glyptolepis*. The finest example of the Canadian *Pterichthys* collected by Mr. Ells had a large piece broken off the left margin when it was found, but with this exception the whole of the upper surface of the helmet and buckler is finely exposed (the plastron being partly covered by the matrix), and the outline of the orbital opening is clearly defined. A few weeks later, Mr. T. C. Weston, also of the Canadian Survey, collected an additional number of fine specimens of the *Pterichthys* from this locality, some of which illustrate admirably the shape, sculpture and mode of articulation of the pectoral spines. Associated with these there are, in Mr. Weston's collection, a nearly perfect but badly distorted specimen of a *Glyptolepis* fully seven inches in length, some fragments of *Psilophyton*, and a spore case of a *Lepidodendron*.

Taken collectively, the specimens thus far obtained of the

* Palæontology, Second Edition, page 141.

Canadian *Ptericthys* show nearly all the characters of the helmet, buckler, plastron and pectoral spines, in the most satisfactory manner, but no vestiges of the tail have yet been detected, nor of any of the fins other than the two pectoral spines. The nature of the mouth and of its dentition, if it had any teeth, are unknown, and the small isolated plate in the orbital cavity (the "os dubium," of Pander, the "median" plate of Owen) has not yet been observed. In the number, outlines and disposition of the plates on the upper and lower surface of the head and body, and in the shape and mode of articulation of the pectoral spines, the Canadian fish agrees, in every essential point, with Pander's well known figures of a typical *Ptericthys*, but the sculpture of the entire surface of the former is precisely like that of *Bothriolepis ornata* Eichwald, which is thus described by Agassiz:* "Les ornemens de cette espèce consistent en petits enfoncemens circulaires placés les uns à côté des autres et séparés par des carènes qui, par leur juxta-position, paraissent hexagonales, à-peu-près comme les vitraux ronds des anciennes fenêtres, avec l'entourage en plomb qui les réunit. Les creux ont à-peu-près la grandeur d'une bonne tête d'épingle, et ils sont placés en séries linéaires plus ou moins régulières, formant des lignes ondulées sur la surface de l'écaille. Pour la plupart, ces creux sont isolés les uns des autres, quelquefois aussi plusieurs se confondent en formant un sillon plus ou moins long. Les carènes intermédiaires sont tranchantes et minces, mais elles se maintiennent au même niveau ; l'on ne pourrait donner une meilleure image de cette sculpture des plaques, qu'en enfonçant des epingles, la tête la première, sur du gyps encore frais, car il en résulterait le même dessin. En examinant ces plaques à la loupe, on voit au fond de chaque cellule osseuse un petit trou central, qui mène dans un canal médullaire de l'intérieur de l'écaille. Evidemment ces trous étaient destinés à donner passage aux fins vaisseaux sanguins qui montaient à travers l'écaille pour se ramifier dans l'epiderme qui couvrait la plaque." All the markings so carefully described in the above passage, even to the minute perforations through the plate in the center of each pit, can be made out with perfect ease in most of the specimens collected by Messrs. Ells and Weston.

The Canadian Ptericthys is so closely allied to the *Bothriolepis ornata* that it is by no means certain whether the two are specifically distinct or not. Apart from its peculiar sculpture, the specific characters of *B. ornata* are very imperfectly ascertained, the species having been founded exclusively on a few large isolated plates of a placoderm, from the Devonian rocks of Russia and Scotland. Until more perfect examples of *B.*

* Monographie des Poissons Fossiles du Vieux Grès Rouge, &c., page 99.

ornata shall have been described and figured, it will be impossible to institute an accurate comparison between it and the nearly related Canadian form. There are, however, good reasons for supposing that the European species attained a much larger size than the Canadian, for Agassiz says that the plates of *B. ornata* are from three to six inches in length, and, judging by this, the approximate length of its helmet and buckler together may be roughly estimated at from six to twelve inches at least. The largest isolated plate of the *Pterichthys* from the Baie des Chaleurs yet obtained (one of the ventro-laterals) is only two inches and a half long, while the smallest of two perfect specimens of the united helmet and buckler from the same locality is a little over two inches in length, and the largest (the fine specimen collected by Mr. Ells) is just six inches.

Under the circumstances, the writer thinks it most prudent to give to the Canadian *Pterichthys* a local and provisional name, with a brief diagnosis of its most salient characters, as follows: premising that a more detailed description of the species, accompanied with figures, will appear at an early date in one of the publications of the Canadian Geological Survey.

Pterichthys (Bothriolepis) Canadensis, Nov. Sp.—Plastron nearly flat. Helmet moderately arched above, most prominent immediately behind the orbital cavity where it rises into a ridge or blunt keel, which is continued, at intervals, with greater or less distinctness, along the median line of the buckler. Buckler slightly arched, median keel strongest in the center of the dorso-median plate, and in the posterior half of the post-dorsomedian. General outline of the helmet and buckler combined elliptic-ovate, their united length being nearly, but not quite, twice the maximum breadth of the buckler. Dorsomedian plate large, hexagonal, apparently rather wider than long; its upper margin slightly concave on both sides and somewhat pointed in the middle, its lower margin being concave. Orbital cavity situated nearly in the center of the helmet, transversely reniform or bean-shaped in outline, much wider than high. Upper margin of the orbital cavity broadly, regularly and very shallowly concave, the lower being correspondingly convex, while the two lateral extremities are symmetrically and rather narrowly rounded.

Pectoral spines extending nearly to the posterior end of the buckler, thin and compressed vertically; moderately broad laterally where they are articulated to the ventro-lateral plate, and widening to about their mid-length, where they exceed the breadth at their articulation by about one line. From this widest point the breadth of the spines is again gradually reduced up to the joint separating the two segments of which they are composed, from whence they taper gradually to an acute point. The two segments are divided, nearly transversely, by a ball and socket

joint, the ball being in the anterior and the socket in the posterior or terminal segment. The anterior end of each spine seems also to be furnished with a ball and socket joint, as there is a strongly inflected cavity in the ventro-lateral plate to receive the anterior end of the spine, which latter terminates in a rounded protuberance. On the inner and outer lateral margin of the pectorals there is a single row of crowded, nearly erect, conical, tooth-like, hollow spines. These are directed towards the articulation of the spine with the ventro-lateral plate up to about the mid-length of the anterior segment, and from thence they begin to point towards the posterior termination of the spine.

Sculpture of the helmet, buckler, plastron and pectoral spines very closely resembling that of the plates of *Bothriolepis ornata*, but much finer and more delicate.

Montreal, July 6, 1880.

ART. XX.—*A new Meteoric Mineral (Peckhamite), and some additional facts in connection with the fall of Meteorites in Iowa, May 10th*, 1879; by J. LAWRENCE SMITH, Louisville, Ky.

THE mineral now named Peckhamite was referred to in a former paper on the Emmet Co. Meteorite.* Having since been furnished with additional material, I have been enabled to make a more positive determination as to its distinctive characters. It is decidedly different from any mineral I have seen associated with meteorites. In two or three specimens it projected above the outer surface, having a dingy yellow color and a fused surface. When broken it has a greasy aspect with a more or less perfect cleavage, and the yellow color has a greenish hue. In structure it differs widely from olivine, as may be seen under the microscope. Not being furnished with the proper instrument, I have not been able to study all its optical characters. Small rounded nodules, several millimeters in size, are found in the interior of the mass, sometimes of irregular form, from which fragments nearly pure can be detached. Its specific gravity, taken with about 300 milligrams of fragments, is 3·23. Chemical analyses from two specimens gave:

	1	2	Oxygen ratio from No. 2.
Silica	49·50	49·59	25·73
Ferrous oxide	15·88	17·01	3·77
Magnesia	33·01	32·51	12·76
	98·29	99·11	

No. 1 was made with 100 milligrams detached by myself; and No. 2 with 350, sent to me by a friend. The oxygen ratio gives very closely the formula $\dot{S}i\dot{R} + \frac{1}{2}(\dot{S}i\dot{R}_2)$ or perhaps more correctly

* This Journal, June, 1880.

$2\dot{S}iR+\dot{S}i\dot{R}_2$, being two atoms of enstatite or bronzite plus one atom of olivine. There was a slight inaccuracy in the statement of the formula in the former paper.

I have thought proper to call the mineral Peckhamite in honor of Professor Peckham, who has been industrious in collecting the minerals of our Lake region, and to whom I am indebted for every facility in prosecuting my researches in connection with this meteorite.

In a supplementary note published in the June number of this Journal, on page 495, I have given a brief account of the discovery, near the border of Dickinson County (the county west of Emmet) and about five or six miles southwest from where the larger masses fell, of evidence that the fall of the meteorite was attended by a shower of fragments, as of hail-stones, falling upon the water of a lake near by. The search which has been made over the region by men, women and children for a distance of eight miles, and one half mile in width, has resulted in the collection of thousands of pieces of this meteorite from the size of a pea to 500 grams, but mostly quite small. The number found has been estimated at 5000, weighing in all about 30 kilograms. All the smaller pieces are little lumps of nickeliferous iron, and even the larger ones have but little stony material attached. Those familiar with the numerous small stones that were collected after the Pultusk fall, have but to imagine these stones to be all metal, and some idea may be formed of what these fragments are like; they are, however, more irregular than the Pultusk stones. These lumps of iron were on the wet prairie for nearly one year, and yet they are not in the least rusted, many parts being bright, some looking like nuggets of platinum. It may be that they are protected by an invisible coat of melted silicate.

It is clear that the rapid passage of the meteorite through the air disintegrated the surface very rapidly, pulverizing the stony part completely; and the nodules of iron not undergoing this disintegration fell in the track of the meteorite for many miles, and the greater number of them will never be found.

I must state that we are indebted to Mr. Charles F. Birge, of Keokuk, Iowa, for collecting these facts, as well as many others in connection with this most remarkable meteoric shower.

In conclusion, I would state that this last discovery enables us to fix more positively the direction of the meteorite. In former descriptions, including my own, the course of the meteorite is given as from northwest to southeast. But its general direction was from south-of-west to north-of-east; the meteorite came from south of an easterly course in Davidson County, and going north of that line in Emmet County, dropped the smaller fragments over the surface of the latter.

ART. XXI.—*The Earth as a Conductor of Electricity;* by JOHN TROWBRIDGE.

THE Observatory of Harvard University transmits time signals from Cambridge to Boston, a distance of about four miles. The regular recurrence of the beats of the clock affords a good means of studying the spreading of the electrical current from the terminal of the battery, which is grounded at the observatory; and the establishment of the Telephone Dispatch Companies in Cambridge, with their various ground connections, gave me a means of studying this spreading. In all the telephone circuits between Boston and Cambridge, in the neighborhood of the direct line between these places, the ticking of the observatory clock could be heard. The ticking heard in the telephones at the various stations has been attributed to the proximity of the telephone circuit wires to the time wires from the observatory. This is evidently an erroneous conclusion, as will be evident from a short mathematical consideration:

The expression for the induction produced in one wire by making and breaking a current in a parallel wire, is

$$R_2 y_2 = \pm M y_1,^*$$

in which y_2 represents the induced current, R_2 the resistance of the circuit which conveys this induced current, M the coefficient of induction between the parallel circuits, and y_1 the current in the primary circuit; the interruption of which produces the induced currents. Now $M = \iint \frac{ds\, ds'}{r}$, in which ds and ds' are elements of the parallel wires, and r is the perpendicular distance between them. The value of M in the case we are considering is $M = \frac{R_1^2}{r}$, in which R_1 represents the length of the parallel wires along which the induction takes place and r is the distance between the wires.

We shall therefore have $R_2 y_2 = \pm \frac{R_1^2}{r} y_1$, eq. (1). Now the electromotive force in the induced current y_2 is very much greater than that of the inducing current y_1, and in order that the current strength y_2 should be able to develop even a small electro magnetic effect in the receiving telephone, the coefficient of induction must be increased, or the distance along which the conductors are parallel, the distance between them remaining the same. An arithmetical consideration of eq. (1) will convince one that with telephones of the resistance usually employed, no inductive effect will be perceived by the employ-

* Maxwell's Electricity and Magnetism, vol. ii, p. 209.

ment of even ten-quart Bunsen cells between wires which run parallel to each other a foot apart for the distance of thirty or forty feet. In order to detect an inductive effect under these conditions, a telephone of three or four units of resistance must be employed. The ordinary Bell Telephone has a resistance from thirty to sixty units. For still stronger reasons it is impossible to hear telephonic messages by induction from one wire to another, unless the two wires between which induction is produced run parallel to each and very near to each other a long distance. This distance generally exceeds the distance at which the ordinary Bell Telephone ceases to transmit articulate speech. The effects which have usually been attributed to induction on telephone circuits are due to the earth connections and to imperfect insulation. There would be no trouble from induction if telephone wires were enclosed in a cable; for a consideration of eq. (1) will make it evident that the telephonic message transmitted over one wire would have no practical effect upon the neighboring wires enclosed with it.

Since the transmission of the time signal service of Harvard College Observatory through all the telephone circuits in Boston and Cambridge is evidently not due to induction, but to tapping, so to speak, the earth at points which are not at the same potential, it was an interesting question to study the extent of the equipotential surfaces formed by the ground of the time signal service circuit at Cambridge and in Boston. I speedily discovered that the time signals could be easily heard in a field an eighth of a mile from the observatory, where one ground of the time circuit is located. The method of exploration was to run a wire five or six hundred feet, to ground it at its two ends in moist earth, and to include a telephone of fifty or sixty ohms resistance in the circuit. On completing the circuit through the telephone and the ground, the evidence of an electrical current was plainly apparent from the ticking which the making and breaking of the circuit produced in the telephone; and the time signals of the observatory clock were distinctly heard. At the distance of a mile from the observatory and not in the direct line between the observatory and the Boston office, the time signals were obtained by tapping the earth at points only fifty feet apart. At a distance of five hundred feet directly behind the observatory, no points five hundred feet apart could be found which were not practically at the same potential. The survey was carried to a distance of a mile behind the observatory ground, with negative results. At points one mile from the central line between the observatory and the Boston office, the time signals could not be heard on the trial wire of six hundred feet. This was to be expected, since the trial wire should have its length increased as the dis-

tance from the grounds of the battery increases, in order to permit of one end of the wire touching a point of higher potential than the other.

The theoretical possibility of telegraphing across the Atlantic without a cable is evident from this survey which I have undertaken. The practical possibility is another question.

At no point in our survey did we find an absence of earth currents. The peculiar crackling noises heard in telephones are due to earth currents and not to fluctuations in the batteries employed on telephone circuits; for they were characteristic of the circuits employed by us in which the earth was used as a part of the circuit, and were absent when a battery circuit was closed without the intervention of the earth. The tick produced in the exploring telephone, whenever the circuit was closed, through the ground was due to earth currents and not to polarization between the copper wire and the moisture of the ground, for it was many hundred times stronger than the polarization effect produced by dipping the copper terminals of the telephone wire in acidulated water. This crackling noise produced by the earth currents in a telephone is a curious phenomenon, and shows that the earth currents have a rapidly intermittent character, which escapes observation by any other instrument than the telephone. A delicate electro-dynamometer, for the registration and observation of these intermittent earth currents, is much to be desired. In some cases the pulsatory effect of these earth currents was very marked. At no point which we explored, were evidences of earth currents absent. They seemed to be more pronounced along water courses.

In a discussion of the earth as a conductor, Steinheil says: "We cannot conjure up gnomes at will, to convey our thoughts through the earth. Nature has prevented this. The spreading of the galvanic effect is proportional not to the distance from the point of excitation, but to the square of this distance; so that at the distance of fifty meters only exceedingly small effects can be produced by the most powerful electrical effects at the point of excitation. Had we means which could stand in the same relation to electricity that the eye stands to light, nothing would prevent our telegraphing through the earth without telegraph conductors. But it is not probable that we shall ever attain this end."*

Theoretically, however, it is possible to-day to telegraph across the Atlantic Ocean without a cable. Powerful dynamo-electric machines could be placed at some point in Nova Scotia, having one end of their circuit grounded near them and the other end grounded in Florida, the conducting wire con-

* Die Anwendung des Elektromagnetismus, p. 172, 2d ed., 1873.

sisting of a wire of great conductivity and carefully insulated from the earth, except at the two grounds. By exploring the coast of France, two points on two surface lines not at the same potential could be found; and by means of a telephone of low resistance, the Morse signals sent from Nova Scotia to Florida could be heard in France. Theoretically this is possible; but practically, with the light of our present knowledge, the expenditure of energy on the dynamo-electric engines would seem to be enormous.

The points made in this paper are as follows:

1. Disturbances in telephonic circuits usually attributed to effects of induction are, in general, due to contiguous grounds of battery circuits. A return wire is the only way to obviate these disturbances.

2. The well-defined equipotential surfaces in the neighborhood of battery grounds shows the theoretical possibility of telegraphing across large bodies of water without the employment of a cable, and leads us to greatly extend the practical limit set by Steinheil.

3. Earth currents have an intermittent character, with periods of maxima and minima which may occur several times a minute during the entire day. This intermittent character is seldom absent.

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SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On Chemical Curves as Lecture-Illustrations.*—The growing importance of chemical dynamics renders it very desirable to use the graphical method in class instruction. In order to make clear the significance of these curves, especially to those who grasp graphic methods difficultly, Mills has contrived an apparatus for lecture use in which a chemical process constructs its own curve under the eyes of the observer. It consists of a series of glass cylinders, filled with water and inverted each in a circular glass trough, also containing water. Behind these cylinders is a corresponding series of glass tubulated retorts, of 100 c. c. capacity, each beak opening beneath the mouth of a cylinder. Provision is made for leading off the overflowed water. The frame of the author's apparatus is 289 cm. long and 51 high, the capacity of each cylinder being about 270 c. c. To show the effect of dilution upon the action of hydrogen sulphate upon zinc, the sheet metal 0·55 mm. thick was cut into pieces 14 mm. square exactly, and rolled into a cylinder. The sulphuric acid, of specific gravity 1·843, was diluted to strengths varying progressively by 3 per

cent in volume and 50 c. c. of each portion was placed in each retort in order. The retorts and the pieces of zinc must be carefully cleansed. The experiment may be conducted to give either the effect of mass of the acid, or of the time of contact; yielding in the first place a quantity-curve and in the second a time-curve; the quantity of hydrogen gas collected in the successive cylinders giving the required curve, the height of the water column being the ordinate in each case. The time-curve is but a single curve, usually of a logarithmic form. The quantity-curve has points of inflection and consists generally of one or more consecutive hyperbolas, showing (1) that the progress of the reaction is not proportional to the acid-strength; (2) that there is a maximum with 27 per cent H_2SO_4, a minimum with 33 per cent and a second maximum with 36 per cent; very weak and very strong solutions being without effect; (3) that the effect of weakening the solution is more rapid than an equal strengthening of it; (4) that the entire process is accomplished in three stages represented by three curves; and (5) that, if the weight of zinc has been such as exactly to fill the cylinder with hydrogen, the water remaining represents the remaining chemical energy of the zinc. By the simultaneous use of blackboard or lantern curves exactly drawn the significance of the method may be clearly proved. For the time-curve, the author prefers the action of acid on zinc and for the quantity-curve of the action of sodium hydrate upon aluminum. —*J. Chem. Soc.*, xxxvii, 453, June, 1880. G. F. B.

2. *On the Estimation of Zinc and Zinc-dust.*—Beilstein and Jawein have suggested a new and simple apparatus for the determination of the purity of zinc and zinc-dust by means of the hydrogen which they evolve by the action of acids. The sample to be examined is weighed into a tube which is then placed in a glass bottle. Hydrochloric acid of specific gravity 1·10 is then poured in beneath the tube in the proportion of 11 c. c. to one gram of zinc, and the mouth of the bottle is closed with a rubber cork through which passes a delivery tube. To this a rubber tube is attached connecting it with a similar tube just passing through the cork of a larger bottle of two liters or more capacity. Through a second hole in this latter cork passes another tube nearly to the bottom of the bottle, this tube being divided into cubic centimeters, and its upper and outer end being turned over so as to deliver the water clear of the bottle. At the bottom of the large bottle is an opening by which the water may be drawn off. Both bottles are placed in water to keep them cool. The large bottle being filled with water and the corks inserted in both bottles, the level of the water within and without the graduated tube is made the same, and then the acid is brought into contact with the zinc by inclining the small bottle. Immediately the hydrogen evolved forces the water out of the large bottle through the graduated tube, into a tared flask in which it is collected, together with the water drawn off from below in order to make again the level the same within and without the graduated tube.

The barometer is now noted, the temperature of the water surrounding the receiving-bottle is observed and the overflowed water is weighed. The water contained in the graduated tube between its first and second level, must be subtracted from that collected in the flask. A sample of commercial zinc which gave 99·80 per cent zinc by Fresenius's method, gave 99·89 per cent by this. A lot of zinc-dust which gave by this method 80·59 per cent of zinc, yielded 80·10 by the method of Fresenius.—*Ber. Berl. Chem. Ges.*, xiii, 947, May, 1880. G. F. B.

3. *On a Reflection Cuprimeter.*—BAYLEY showed in 1878 that the light transmitted by dilute solutions of cupric salts is deficient in those rays which the spectrum of light reflected from metallic copper has in excess, and hence that if we look at a copper surface through a sufficient thickness of copper sulphate solution, the metal appears silver white, the solution absorbing the excessive rays which make the copper red. He has now perfected an instrument for the estimation of copper, founded upon this principle, which he calls a reflection-cuprimeter. A copper mirror movable about a horizontal axis reflects the direct light of the sky vertically upward through two tubes of glass closed at bottom by glass plates and having at top convex lenses by which the light is concentrated upon two paper discs made translucent by glycerin. The whole is surrounded by a wooden case to shut out extraneous light. By means of lateral openings near the lower ends of the tubes, these are put in communication with reservoirs, one containing a standard solution of copper, the other the solution to be tested. The former is made by dissolving one gram of pure copper in nitric acid, adding a slight excess of sulphurous acid and diluting to a liter at 15° C. Covering one-half the mirror with silver, the other half was looked at through a varying depth of the solution until both disks were equally white. The length of the column required was then noted, the tubes being graduated for this purpose. In the author's instrument it was 8·01 cm. Since the length of the column required to produce white light is inversely as the quantity of copper present and since a solution of copper containing 0·1 per cent required a length of 8·01 cm., a solution of copper containing 100 per cent of metal, i. e., pure copper, would require a thickness of 0·0801 mm. This length the author calls a "cuper;" and hence 1000 cupers of the standard solution produces a white disc and this solution represents copper 1000 times diluted. If c is the number of cupers thick the solution is when the disc is white, c will also represent the volume of liquid in c. c. which contains one gram of copper and $\frac{1000}{c}$ the amount of copper in grams contained in one liter of solution. With a solution containing 0·801 gram of copper per liter six readings of the cuprimeter gave ·806, ·803, ·800, ·793, ·793 and ·813; or ·803 as a mean. Iron, in the ferrous condition, is not injurious. The substance to be analyzed, in such quantity as is supposed to give a gram of copper, is dissolved in nitric acid,

treated with sulphurous acid in excess, boiled, cooled, made up to a liter, and examined in the cuprimeter.—*J. Chem. Soc.*, xxxvii, 418, June, 1880. G. F. B.

4. *On a Method of producing Acetal.*—In the hope of producing in larger quantity, a crystalline substance which they had observed, ENGEL and DE GIRARD dissolved aldehyde in about its own volume of absolute alcohol and passed into it a current of hydrogen phosphide gas for three days, the mixture being at first cooled to — 40°, then to — 21°. No crystals were formed; but on adding water a liquid separated, which gave on fractioning a liquid boiling at 104°, having an ethereal odor, a specific gravity of 0·829 at 13°, and a vapor density of 4·3. Moreover, it possessed in other respects the characters of acetal. The yield is considerable. The authors are now investigating the conditions of maximum production.—*Bull. Soc. Ch.*, II, xxxiii, 457, May, 1880. G. F. B.

5. *On Phlobaphen and Oak-red, and their Relation to Tannin.*—BÖTTINGER has proved that, under the influence of sulphuric acid, quercitannin splits into sugar and oak-red, and that this latter body is identical with the phlobaphen of Stähelin and Hofstetter. To prepare phlobaphen 22 kilos. of oak tan was extracted, first with ether to remove fat, wax, chlorophyll and gallic acid, then with alcohol. The clear brown solution was evaporated on the water-bath, and left a semi-solid mass having a strong odor of tan. After a second extraction with ether, the lumpy residue consisted essentially of two constituents — tannin, soluble in water and phlobaphen, insoluble in it. Their complete separation was extremely difficult, however, owing to the fact that the latter is soluble in a solution of tannin. By repeated extractions with water or alcohol, both of these bodies were finally obtained pure. The quercitannin, treated in concentrated solution with cold dilute sulphuric acid, becomes a thick mass which liquefies on gentle heating. At higher temperatures decomposition takes place, a compact brownish mass of oak-red is deposited and sugar remains in solution. Oak-red and phlobaphen are identical in their physical properties and in their behavior to oxidizing agents, zinc dust, fused potash, acetic oxide, benzoyl chloride, fuming hydrochloric acid, etc. As purified it is a reddish-brown powder, extremely insoluble in the ordinary solvents, but soluble in a tannin solution. It is blackened by ferric chloride, oxidized readily to carbon dioxide and water, gives no result on boiling or heating with zinc-dust, gives protocatechuric acid when fused with potash, and affords the formula $C_{14}H_{10}O_6$. Acetic oxide produces a triacetyl derivative, benzoyl chloride a tribenzoyl compound. By the action of fuming hydrochloric acid carbon dioxide and water separate, and a brilliant black body results apparently identical with that obtained by similar treatment from pyrogallol. The author concludes that while quercitannin and phlobaphen both act efficiently in the tanning process, yet that it is to the latter substance that the peculiar result is essentially due.—*Liebig Ann.*, ccii, 269, May, 1880. G. F. B.

6. *On the Occurrence of Globulin in Potatoes.*—ZÖLLER has called attention to the existence in potatoes of one of the albuminoid substances called globulins by Hoppe Seyler. The finely divided potatoes are rapidly washed free from starch and soluble matters, the pulp is pressed, and treated with a ten per cent solution of common salt. In the solution, made neutral with a few drops of a one per cent solution of Na_2CO_3, a lump of rock salt is hung, and the globulin separates in white flocks. It contains 14·2 per cent of nitrogen and its properties prove it to be quite similar to one of the myosins of muscular tissue.—*Ber. Berl. Chem. Ges.*, xiii, 1064, June, 1880. G. F. B.

7. *On the behavior of Carbonic Acid in relation to Pressure, Volume and Temperature.*—Professor R. CLAUSIUS discusses the bearing of recent observations upon the departure of gases from the law of Marriotte and Gay Lussac, and advances a theory to account for these departures of a gas from the perfect state. In a perfect gaseous state the molecules rush together and separate completely after the collision. When the gas is condensed to a liquid the molecules are generally held together by their mutual attraction, and separate only under certain favorable conditions. Between these extreme conditions one can imagine an intermediate state in which the molecules do not completely separate, but oscillate about one another while moving onward as a whole until they are separated by further collisions. The number of these pairs of attached molecules would be greater the lower the temperature, and hence the less the mean *vis viva* of the motion; on a further fall of temperature more than two molecules might collect together and execute the progressive movement in common. On the above assumption the mean strength of the mutual attraction of the molecules would be increased since the united molecules on account of their greater nearness would attract each other more strongly. "*Hence it would not be allowable to regard the quantity which represents in the formula the mutual attraction of the molecules as independent of the temperature, but one would be obliged to admit that it becomes greater with falling temperature.*" Clausius gives a formula which embodies the above ideas and finds a satisfactory agreement between the values calculated from the formula and those obtained by experiment. At the close of his paper Clausius discusses the pressure curve given by Andrews and James Thomson for the temperature 13°·1, and discovers a reversible cyclical process, and states "that when the theoretical pressure-curve corresponding to the homogeneous state is given, the position of the horizontal straight line answering to the actual processes of vaporization and condensation is also determined. The point made by Clausius has an important bearing upon the subject of thermodynamics and can be fully understood only by reference to his diagrams.—*Wied. Ann.*, vol. ix, p. 337, 1880; *Phil. Mag.*, vol. lviii, p. 393, June, 1880. J. T.

8. *Kerr's experiments on the relation between Light and Electricity.*—H. W. C. RÖNTGEN repeats the experiments of Dr. Kerr

with more perfect apparatus than that used by the latter and employs Nichols' prisms, which gave a much larger field than those used by Dr. Kerr. The results of the latter are confirmed and amplified. Glycerine, sulphuric ether and distilled water show an influence of electricity upon the light transmitted through them. Some interesting diagrams are given which show the appearance presented when the liquid under examination is experimented upon. The liquid was also set in movement and the author states that the movement of the particles of the fluid have a marked influence upon the conditions of the directions of the vibrations of the light.— *Wied. Ann.*, No. 5, p. 77, 1880. J. T.

9. *Contributions to Molecular Physics in High Vacua.*—In a paper which is a continuation of the Bakerian Lecture on the illumination of lines of molecular pressure, read before the Royal Society, December 5, 1878, Mr. CROOKES describes some new and remarkable experiments which confirm the theory that the phenomena described by him are due to the repulsion of the molecules of the residual gases after they had obtained a negative charge by striking upon the negative pole. The negative pole not only gives the initial charge but continues to act upon the molecules and causes them to move with an accelerating velocity. It is shown that an idle pole placed between the points of discharge in the exhausted tube is charged positively by the impact of the molecules driven from the negative pole. In order to prove that the action between two streams of molecules is due to the mutual repulsion between the electrified molecules and not to the electrical action between the electrical current carried by the molecules, an exhausted tube was provided with two negative poles and one positive pole. The two molecular streams, instead of converging strongly upon the positive pole under the influence of the mutual attractions of the electrical currents, diverge under the influence of the molecular repulsions. Experiments are described which prove that external magnetic influences act upon the electrified molecules differently in low exhaustions and high exhaustions. The magnetic rotations are fainter in low vacua and depend as much upon the direction of the induction spark as upon the pole of the magnet presented to the discharge.

The phosphorescent effects of the molecular impacts constitute the most beautiful effects obtained by Mr. Crookes. The molecular rays in high vacua possess remarkable powers of causing bodies upon which they fall to phosphoresce. A deadening effect produced upon glass by long continued phosphorescence is confirmed by various experiments. The image of a cross was stencilled by phosphorescence on the end of a large bulb and the bulb was afterward melted and drawn out at the end and afterward blown again into shape. The cross which had produced the image was displaced, and on passing a discharge through the bulb the ghost of the first image reappeared, showing that it had survived the melting of the glass.—*Nature*, June 3 and June 10, 1880. J. T.

10. *On the law of fatigue in the work done by men or animals.*—The Rev. Dr. Haughton, of Trinity College, Dublin, has recently brought to a conclusion a series of papers on Animal Mechanics published in the *Proceedings* of the Royal Society. The ninth of these papers was appointed the Croonian Lecture for the present year, and the tenth paper closes the series.

The most important subject involved in these papers is the experimental determination of the law that regulates fatigue in men and animals, when work is done, so as to bring on fatigue.

Many writers, such as Bougeur, Euler, and others, have laid down mathematical formulæ, connecting the force overcome with the velocity of the movement; but these theoretical speculations have never received the assent of practical engineers.

Venturoli points a method of observations and experiments which would serve to determine the form of the function which expresses the force in terms of the velocity, after which a few carefully planned experiments would determine the constant coefficients; and he adds that "such a discovery would be of the greatest usefulness to the science of mechanics, upon which it depends, how to employ, to the greatest possible advantage, the force of animal agents."

Dr. Haughton believes that he has found the proper form of this function, by means of experiments, and sums it up in what he calls the *Law of Fatigue*, which he thus expresses:—

The product of the total work done by the rate of work is constant, at the time when fatigue stops the work.

If W denote the total work done, the law of fatigue gives us—

$$W\frac{dW}{dt} = \text{const.}$$

or

$$\frac{W^2}{T} = \text{const.} \qquad (1)$$

The experiments made by Dr. Haughton from 1875 to 1880 consisted chiefly in lifting or holding various weights by means of the arms; the law of fatigue giving, in each case, an appropriate equation, with which the results of the experiments were compared. When the experiments consisted in raising weights on the outstretched arms, at fixed rates, the law of fatigue gave the following expression—

$$(w + \alpha)^2 n = A \qquad (2)$$

where w, n, are the weight held in the hand, and the number of times it is lifted, A is a constant to be determined by experiment, and α another constant depending on the weight of the limb and its appendages.

The equation (2) represents a cubical hyperbola.

The *useful work* done is represented by the equation—

$$w\,n = \frac{A\,w}{(w + \alpha)^2} \qquad (3)$$

This denotes a cuspidal cubic, and the *useful* work is a maximum,

when $w = \alpha$, or the weight used is equal to the constant depending on the weight of the limb and its appendages.

When the weights were lowered as well as raised at fixed rates, and no rest at all permitted, the law of fatigue became—

$$\frac{n(1 + \beta^2 t^2)}{t} = A \qquad (4)$$

where n, t, are the number and time of lift, A is a constant depending on experiment, and β is a constant involving the time of lift (τ) at which the *maximum* work is done.

Equation (4) denotes a cuspidal cubic.

When the weights are held on the palms of the outstretched hands, until the experiment is stopped by fatigue, the law becomes—

$$(w + \alpha)^2 t = A \qquad (5)$$

where t is the whole time of holding out.

This equation denotes a cubical hyperbola.

The *Law of Fatigue* seems, in itself, probable enough, but of course its real value depends on its agreement with the results of experiment.

If W denote the total work done and R the rate of work, the law becomes, simply—

$$W \times R = \text{const.} \qquad (6)$$

If different limbs, or animals were used, each working in its own way, and under its own conditions, the *Law of Fatigue* would become—

$$WR = W_1R_1 + W_2R_2 + W_3R_3 + \&c. \qquad (7)$$

and the problem for the engineer would be, so to arrange the work and rate of work of each agent employed, as to make the *useful work* a maximum, the work both useful and not useful, in all its parts, remaining subject to the conditions imposed by equation (7).

In using equation (5) in his concluding paper, detailing the results of experiments made on Dr. Alexander Macalister, Dr. Haughton treats α as an unknown quantity, and finds from all the observations its most probable value to be—

$$\alpha = 5{\cdot}68 \text{ lbs.}$$

This result was compared with that of direct measurements made on Dr. Macalister himself, and indirect measurements made on the dead subject, from all of which Dr. Haughton concluded the value of α to be—

$$\alpha = 5{\cdot}56 \text{ lbs.} \pm 0{\cdot}125 \text{ (possible error).}$$

This result agrees closely with that calculated from the law of fatigue.

It should be added that a proposal was made by Dr. Haughton to Dr. Macalister to make the experiment conclusive by direct amputation of his scapula, a course which he, unreasonably, objected to, as he draws the line of "vivisection" at frogs.—*Nature*, xxii, 554.

11. *Acceleration of Gravity at Tokio, Japan.*—The paper by Messrs. Ayrton and Perry on a "Determination of the Acceleration of Gravity for Tokio, Japan," published in the April number of the Philosophical Magazine and alluded to on page 130 of this number, has been severely criticized by Major Herschel (l. c. June, 1880, p. 446). A reply to this criticism is given by the authors in the July number of the same journal, p. 43.

12. *Winchester Observatory of Yale College: Circular of the Horological and Thermometrical Bureaus*, June, 1880.—These bureaus have been established by the corporation of Yale College, at the recommendation of the Board of Managers of the Winchester Observatory. The former is designed to encourage the higher development of the horological industries, and to pursue researches calculated to aid in the construction of refined apparatus for the measurement of time. This circular gives detailed information in regard to the tests to which time pieces are subjected; the regulations of the distribution of the time service from the observatory, a list of the standard instruments employed in rating time pieces, and those used in testing thermometers. Watches and chronometers are submitted to well considered tests during varying times, and certificates are given which embody the performances of the time pieces and the conditions of the tests. In the work of the thermometrical bureau, thermometers are carefully compared with the air thermometer and with Kew standards. The circular also contains a synopsis of the constants of the Kew, Fastré and Casella primary (mercurial) standards. The details connected with these bureaus have evidently been carefully considered and ably carried out. A field of great usefulness is open to this department of the Winchester Observatory.

Physical Science in America, also, cannot fail to be indebted to it. The late report of the Astronomer Royal, Airy, speaks of the time service as an important feature in the work of the observatory of which he is director, and the French Astronomers in many recent articles have directed public attention to the importance of work similar to that undertaken by the Horological Bureau of Yale College. The thermometrical bureau promises to afford valuable assistance to the United States Signal Service and to meteorology in general, also to medico-physiological investigations and to many departments in the arts. J. T.

II. Geology and Natural History.

1. *Odontornithes; a Monograph on the Extinct Toothed Birds of North America;* by Professor O. C. Marsh, pp. i–x, 201, 4to, thirty-four plates, with Appendix containing a Synopsis of American Cretaceous Birds. 1880. Advance copy, issued with the permission of the Chief of Engineers.—This most valuable and original memoir, which forms volume VII of the Survey of the 40th Parallel, will receive the notice which its importance demands in a future number of this Journal.

2. *Mucorini announced as the chief source of Mineral Coal.*—Professor P. F. REINSCH, of Erlangen, Bavaria, has announced, as the result of some recent investigations of mineral coal by means of thin transparent slices, the view that in the formation of coal only certain low forms of plant-life took part; and that these forms are so well preserved in some cases that almost no difference can be observed in their most intimate structure from similar forms still living. They are long fibrous forms of cellular structure, united to strong stems; and in the fibrous net-work, other forms, spherical in shape are imbedded, organically connected with the net-work. The fibers in the coal, which, according to the author's conclusions, constitute the chief part of it, consist, as a general rule, of opake coaly material, but in the finer ramifications a micro-granular structure can be distinguished. The spherical bodies, which sometimes adhere together with flattened sides, have a diameter of from 0·13 to 0·24 millimeter. The structure is somewhat more sharply defined after heating the section with caustic potash. In polarized light these forms behave precisely like starch. In revolving the Nichols the revolving dark cross appears very plainly in all the spherical bodies. There is generally at the center a rhombohedral kernel of a somewhat more transparent substance than the rest; and the substance in them which becomes somewhat more transparent on treatment with caustic potash has a radiated structure. The forms are declared to be gigantic Mucorini.

These conclusions of Professor Reinsch are wholly opposed to those of other investigators, and to the facts presented by all the ordinary kinds of mineral coal. The author has evidently misunderstood the objects under examination, and his supposed facts are not likely to find acceptance in science.

3. *A New American locality of Fergusonite;* by W. E. HIDDEN (communicated). Besides other results of my search for platinum, in the auriferous gravels of the Southern States, is the discovery, in July, 1879, of the mineral fergusonite at Brindletown, Burke Co., N. C. A few of the crystals were sent by me lately to Dr. J. Lawrence Smith, who answers as follows: "I received the fergusonite safely and have made an examination of it. It is beyond all doubt that mineral; its specific gravity is 5·87.

Metallic acids, principally columbic acid,	49·83
Yttria earths,	47·01
Oxide of iron and uranium,	·42
Water,	1·01
	98·27

This analysis corresponds perfectly with fergusonite. I intend of course, to make a more thorough examination on well selected pieces. It also possesses the property of glowing in an eminent degree." A more particular description with figures of crystals and complete analysis will be given later.

4. *DeCandolle's Phytography. La Phytographie, ou l'Art de decrire les Végétaux considérés sous différents points de vue, par* ALPH. DECANDOLLE, etc. Paris, Masson, 1880. pp. 484, 8vo.—Valuable as the present volume is, it may very probably not be

translated into English. So we propose to give a running account of its contents, adding here and there some brief comments, critical or otherwise. Treatises like this can be written only by botanists of long experience; and long experience, founded upon good training and accompanied by good judgment, gives the right to speak with a certain authority, particularly upon writing and publication in systematic botany, in which rules and method are most important. DeCandolle is now one of our oldest systematists, one who as editor as well as author has had to consider every sort of phytographical question; and the volume he has here produced is a needful supplement to the Philosophia Botanica of Linnæus and the Théorie Élémentaire of the elder DeCandolle, the two classical books which the serious botanical student should early and thoroughly master. Phytography has to do with form and method in botanical works; and natural history is nothing if not methodical. Its advancement by research and its educational value—which will be more and more appreciated as it is better taught—both depend upon correct morphology and upon well-settled method. Those who will not use its proper language and respect its customs, must not expect to be listened to, any more than is unavoidable. Observation and interpretation must go together, if either is to be of value; the naturalist must not only observe that he may describe, but describe if he would observe. In his preface DeCandolle remarks upon the peculiar advantage of natural-history study in the combination of observation with judgment, and upon the importance to a student of acquiring a clear idea of what natural groups are, what a natural classification and the subordination of groups really mean, and how a naturalist arranges, names, and with precision defines the immensely numerous objects of his study. Men who have distinguished themselves in various professions and lines of life, have owned the advantage they have derived from this kind of training in youth, even though they never became naturalists.

DeCandolle's book is in thirty chapters, many of them short and somewhat discursive, and generally abounding in recommendation and advice, rather than laying down positive rules.

The first chapter glances at "the evolution of botanical works" from Cesalpino, with whom scientific botany began in the middle of the 16th century, to Linnæus, whose rules and spirit still govern, and to our own times,—noting the gradually increasing importance of herbaria as compared with botanic gardens. The second chapter touches upon the moral and intellectual dispositions necessary in botanical work, and asks the question what manner of men botanists are or ought to be. As their pursuits do not lead to fortune, and professorships are neither numerous nor well paid, he concludes that botany is just the science for disinterested people to prosecute from pure love of knowledge and the pleasure of discovering something new; that it does not deal with questions of a very high order, nor require very difficult or absolutely rigorous reasoning. The faculties which it brings into

requisition are the spirit of observation and of order, sagacity, and a certain good sense in the appreciation of facts; that, if it does shine with great éclat, at least the faults of its cultivators are not likely to harm any one; that, equally with the other sciences, it tends to elevation of character in that it requires an ardent love of truth, reposing as it does upon the idea that the veracity of its cultivators is absolutely complete. He concludes, "Les sciences jouent dans le monde le rôle d'une école practique de bonne foi. D'apres ces réflexions, il est permis de penser que les botanistes sont ordinairement et devrait être toujours des hommes paisibles, inoffensifs, indulgent pour les erreurs de leur confrères, et occupès bien plus de l'avancement de la science que de leurs intérèts ou de leurs petites glorioles. . . . Ne nous arrêtons pas cependant sur de rares exceptions. La presque totalitè des botanistes est pénétrée du sentiment de la justice et des convenances. On en trouverait difficilement un seul qui ne reconnût le principe fondamental de ne pas faire à autrui ce qu'on ne voudrait pas qui vous fût fait."

Still—our author continues—sometimes the perfectly honest and right-minded botanist may have failings. He may, for example, neglect to cite his predecessors, or cite them inexactly, either from negligence (not to speak of calculated omissions, which show want of honesty and soon bring down reprobation), or from the want of literary resources. The latter case may be deemed a misfortune, and no fault. But, our author rejoins, if he has not the necessary books within his reach, why not go where they are and consult them? Or if unable to do that, why need he publish?

Some good advice follows about polemics and captious criticism; which we pass over, as seemingly superfluous, so long as the botanists are almost without exception such peaceable and good people. Something is said of the need of a right appreciation of the extent of the science; of the danger of exclusive devotion to a single branch of botany, in which one may lose all just perspective; and, finally, of what accuracy means in natural history as distinguished from mathematical exactness. Everywhere the naturalist has to judge as well as to measure.

The third chapter discourses upon the manner of preparing and editing botanical works, and the most advantageous modes of publication, considers the different degrees of publicity:—for instance, complete and durable publicity is attained when a monograph of an order or genus, a flora, a Species or a Genera Plantarum is published and placed on sale by the booksellers; or when an article or memoir is contributed to any leading and well-known botanical journal, or to the bulletin of a purely botanical society, which publishes with some regularity and indexes its volumes; or when printed in the transactions or bulletins of any scientific society, if separate copies in sufficient number are printed and fairly distributed or placed on sale. The usage in some learned societies of paging each memoir separately and placing it separately on sale is referred to, with implied commen-

dation. Let us add that in all such separate issues, the original pagination of the volume should be scrupulously preserved; and it were better that there should be no other. Less complete, but durable publication is that of *ouvrages de luxe*, so limited in number of copies, and so high in price that only a few libraries can possess them; also articles in journals without full indexes, or with indexes only to a series of volumes.

Incomplete publicity is given when papers upon botany are inserted in the voluminous transactions of general learned societies, of which few individuals can possess the series or find room for them; also articles in reviews, encyclopædias, and the like, treating of many or of all sciences. Even journals of natural history alone fall under the ban, unless divided into separate parts for zoology and botany,—as is the long-continued Annales des Sciences Naturelles of Paris, and the Journal of the Linnean Society of London. A remedy or alleviation of these obstacles to publicity, and of others like them, is supplied by the catalogue of papers published by the Royal Society of London; which noble work was instigated by the late Professor Henry; yet, as this embraces all sciences and fills a goodly series of volumes, it can seldom be in the library of botanists.

In speaking of the obstacles to scientific publicity which are interposed by too limited editions, high prices caused by undue luxury in plates, and inopportune or inappropriate media of publication, DeCandolle refers to customs in the book trade and in government patronage which need reform; and mentions incidentally what a botanical library costs. He says there should be by the side of every great herbarium and every considerable botanic garden, a special botanical library, without which it is impossible to determine exactly the plants of the one or the other, or to write any good monograph or flora. Such a library costs fifty or sixty thousand francs (ten or twelve thousand dollars), and needs about 12,000 francs for annual purchases. He asks how many such establishments there are in the world, and concludes that there may perhaps be between ten and twenty.

The section on the comparative superiority of certain kinds of works, sets forth the greater value of *books* or systematic works as compared with memoirs or articles.

The language to be employed in botanical publications is the topic of a special article. For descriptions, Latin, and the Latin of Linnæus. "Le Latin des botanistes n'est pas cette language obscure et à rèticences de Tacite, obscure et à periodes pompeuses de Ciceron, obscure et à grâces tortillées d'Horace, qu'on nous fait apprendre au collège. Ce n'est pas même le langage plus sobre et plus clair d'un naturaliste tel que Pline. C'est le Latin arrangé par Linné à l'usage des descriptions et, j'oserai dire, à l'usage de ceux qui n'aiment ni les complicationes grammaticales, ni les phrases disposées sens dessus dessous, ni les parenthèses enchâssées dans les phrases."

This for descriptions, except in local floras, where popular use

demands the vernacular; and we interpose the remark that English botanical language, freely incorporating as it does all Latin and Greek terms, comes next to Latin in convenience, compactness, and facility to all foreign botanists, who, being familiar with Latin, can seldom be at a loss. For discussions and reasonings, the botanists of each nation prefer their vernacular tongue; but DeCandolle would restrict them to the four modern languages, one or two of which, beside his native tongue, every naturalist is now-a-days supposed to be able fairly to read; English, German, French, and Italian. Indeed, DeCandolle recommends Latin and the technically descriptive style even for *generalia*, on the ground of brevity; and he aptly suggests that the less capable botanists are of handling other than Linnæan Latin, the more brief, sententious, and strictly to the point their exposition will be.

Hints are given as to the best mode of collecting literary material, making and preserving notes (each upon separate slips of paper), upon the importance of adding clear explanations of drawings at the time they are made; and upon the desirability of refraining from publication until the work is thoroughly completed, but of then publishing as soon as possible. A manuscript work is said to have its maximum value at the moment of completion. Our author declares that second and third editions are seldom equal to the first. That depends. He objects also to posthumous publication, citing Roxburgh's Flora Indica, published by Wallich, Plumier's plates published by Burmann, and the wretched figures of Velloso; and he might have referred to the ill-advised printing of Griffith's rough notes and comments. But all depends upon the character of the manuscript and the length of time which has elapsed.

Chapters IV–XI traverse the whole subject of descriptions, under various aspects and a rather minute division of topics. As even a brief analysis would overpass available space, we will merely touch here and there upon certain points.

As to the relation of varieties to species, there are two modes of presentation, both of which have been followed by Linnæus, and by most systematists, upon different occasions. Varieties are commonly designated by the small letters of the Greek alphabet, α, β, γ, etc., and also by names when they are pretty distinctly marked. Either the varieties, one or more, may be appended to the species (that is, to the form taken as the type, which usually must be the form originally described under the name), and therefore be treated as aberrant forms; or else the species is characterized as a group of forms, which forms are classified and defined just as species are under their genus. For instance, *Mentha Canadensis* is held to comprehend both a hairy and a smooth form, the two differing also somewhat in other respects. Linnæus founded the species on the former; and it is pretty well agreed that we are to refer the species back to him, however it be limited. Now we may either give a common character to the

species, and then distinguish var. *α. villosa*, and var. *β. glabrata;* or we may characterize the species in general upon the originally named form, and append the variety *β. glabrata.* Either mode has its advantages and is likely to be employed in certain cases. The former classifies the varieties under the species, perhaps more naturally, and exhibits the polymorphous character of what we call a variable species; and DeCandolle considers that it will prevail in proportion as the forms of a species come to be well known; the latter holds closer to the bibliography. There is danger of some misunderstanding when the two modes are used in the same work. In the Synoptical Flora of North America, the former mode is invariably adopted, partly on the score of brevity. Either the originally described form, or a medium or common form is taken as the type, and the varieties are treated as departures from this. Even when the specific character is drawn so as generally to cover the varieties (as should be done as far as possible), some form, and the history of the species generally indicates what form, is kept in view as the norm or alpha. Of course, except for cultivated plants, there is no knowing and no pretence of determining which was the parent form, or in what order the several varieties may have diverged from a pristine stock.

As DeCandolle points out, there is much ambiguity and looseness in the use of this word *type,* which it would be well to avoid. Properly the type of a species *is* the species or genus, or the full idea of it, which no one individual or species may embody, which in the case of a group no single representative or member can fully exemplify. To apply the term to a form which well exemplifies the essential characters of the species or genus is quite natural, and hardly involves any confusion. But the term is also used in a historical sense, as referring to the particular form on which a species was founded, or the species on which the genus was characterized or which its founder had mainly in view, but which very often proves not to be the best representative of the group, sometimes not even a fair one. Finally a particular specimen which the original author described, or an authentic specimen, is said to be a type, or a typical specimen; and this DeCandolle objects to. But after all, such terms can hardly be held to a single sense in technical any more than in ordinary language. Something must be left for the context to determine.

In drawing up the characters of groups, such especially as orders and genera, are exceptions or what we call exceptions to be indicated in the character, or shall this express only what is generally true? DeCandolle discusses the question, but leaves it, as must needs be, for practical judgment to determine. On the one hand the point or the usefulness of a character is blunted or dissipated by the intercalation of alternatives and exceptions, yet characters must be somehow made to correspond with the facts. The method of Bentham and Hooker, of a separate specification of the principal known exceptions, is commended.

Should outlying or anomalous groups be incorporated with the orders they most resemble, or be merely appended as "genera affinia," and the like? The latter was inevitable in the earlier days of the natural system; but increasing knowledge, as well as considerations of symmetry and convenience, more and more fixes the place of these floating groups; so that their general incorporation into the orders by Bentham and Hooker in the Genera Plantarum of our day is in the natural course of things. But botanists have to remember that many of them are still riddles.

DeCandolle classes descriptions under the two general heads of *developed* and *abridged*. A developed description is a detailed account of the whole conformation, without regard to *differentiæ*. The type of an abridged description is the diagnosis, such as the specific phrase, or as Linnæus called it, the *nomen specificum;* what we now universally term the specific name being his *nomen triviale*. In the course of phytography both these have become rare or of special use as regards species, and a hybrid between the two has been engendered which is more serviceable than either. The long and independent descriptions of the olden time are now seldom written. Except for special cases, the development of the natural system in its subordination of groups in ever increasing numbers and definiteness, has rendered them superfluous. What was once stated in the developed description of a species in one formula, and a vast deal more, is now parceled out among the ordinal, tribal, generic, sub-generic or sectional, sub-sectional and other characters, each of which deals primarily, if not wholly, with *differentiæ*. The characters of each grade, being diagnostic, may be comparatively short; but taken together they become almost exhaustive. But to avoid going again over the same ground, subsidiary matters not diagnostical, yet needful or useful, are not rarely intercalated among the more essential points, instead of being collected in a separate paragraph. Consequently the specific diagnosis may be prolonged and get to partake of the nature of a developed description. The remedy for over-length is to multiply divisions and sub-divisions between the genus and the species. To do this well, to arrange the species group within group most definably as well as most naturally, tasks the powers and the patience of a systematic botanist, and tests his aptitude for discerning affinities, and solving practical difficulties.

Developed descriptions are in place in such general works as DeCandolle's *Systema* (which was soon overweighted and crushed by them), and above all in monographs of orders or genera. In his sixth chapter, devoted to this topic, the author cites, in order of date, the principal monographs of orders or tribes (excluding those of a single genus) which may be taken as models (about two dozen only), and points out some of their merits or defects. The subject of abridged descriptions is taken up in chapter VII; and this connects itself with a great number of subsidiary questions and particular details, running on through twenty chapters more (individually short), and forming the most practically useful

part of the book. There are so many points which it were well to call attention to, or sometimes to comment upon, for which space is now wanting, that we must defer the remainder of this critical notice to the next issue of the Journal.

It is to be regretted that, for the completeness of this work the author did not comprehend in it the subject of nomenclature of groups—an important part of phytography—and reprint in it his opuscula, entitled *Lois de la Nomenclature Botanique*, along with some further commentaries, such as his experience and some adverse criticisms from an opposing school may have suggested. This may still be desired, although the little treatise has already been widely disseminated in three languages, and although, as the author incidentally remarks, his own view is shared by an immense majority of descriptive botanists. A. G.

5. *The Ferns of North America: Colored Figures and Descriptions with Synonymy and Geographical Distribution, etc.;* by DANIEL CADY EATON, Professor of Botany in Yale College, 1879, 1880. 71 plates. Published by S. E. Cassino, 299 Washington street, Boston.—From time to time we have noticed this truly classical work, and have now only to congratulate author, artists, and publisher, also the botanical public and the numerous amateur fern-people, upon its happy completion. The author's part has been well sustained throughout. The illustrations began well, and went on better and better. The author adds to the second volume a complete Synopsis of the Genera and Species contained in the work, in systematic order, with diagnostic characters, a most convenient and useful addition. Would that our other Cryptogamia, or any of them, were similarly provided for. A. G.

6. *Index perfectus ad Caroli Linnæi Species Plantarum, nempe earum Primam Editionem (anno 1753), Collatore,* FERD. DE MUELLER. Melbourne, 1880. pp. 40, 8vo.—Baron von Mueller some time ago called attention to the fact, that most botanists cite the second edition of the Species Plantarum of Linnæus instead of the first, thus not really beginning at the beginning. Attributing this practice to the scarcity of the first edition, he now prints an index to that edition, for the use of those who possess only the second, or its equivalent, the third or Vienna reprint. We agree that it is most proper to cite the *editio princeps* for the species the names of which originate there; and, taught by Robert Brown forty years ago, we have uniformly done so. But we suspect that the prevalent habit of citing only the second edition did not grow so much out of the scarcity of the first as from the idea that the revised edition was the properest to use. Still the only safe and true way is to begin at the beginning; and it sometimes happens that the accretions in the subsequent edition are misleading. A. G.

7. *Catalogue of North American Musci;* arranged by EUGENE A. RAU and A. B. HERVEY. Taunton, 1879. pp. 52, 1880.—The species are arranged after Schimper's Synopsis; gen-

eral habitat or geographical range is given; the printing and proof-reading appear to be well done; and "the compilers [may well] believe that they are supplying a want which has long been felt by American botanists." We hope and may expect that we shall have something more than a compiled catalogue before long, although the death of Mr. Sullivant has greviously postponed the desired consummation. A catalogue like this is always useful.

A. G.

8. *Botanical Exploration of the little-known West India Islands.*—Baron Eggers, the commander of the Danish forces in the Danish West Indies, resident at St. Thomas, has issued a circular inviting subscribers for sets of botanical specimens, sections of woods, fruits, etc., in view of a complete investigation of the botany of those West India Islands which have been very little explored, at least in the present century, and which doubtless contain much that is new to science. Hayti and Dominica are particularly in view, also Porto Rico. It is intended that the specimens shall be well studied before distribution, named, and made up into uniform sets. As announced in the circular, the price of botanical specimens was fixed at $12.50 the hundred for Phænogams, and $10 for Cryptogams. But we understand that if encouragement is offered for the making of a considerable number of sets, the price of phænogamous plants will be reduced to the customary $10 the century. Botanists should communicate promptly with Baron Eggers, St. Thomas, Danish West Indies. A. G.

9. *Note to Dr. C. A. White's paper in volume* XX *of this Journal;* by R. Ellsworth Call. (Communicated.)—In Dr. C. A. White's interesting communication to this Journal, vol. xx, July, 1880, pp. 44–49, "On the Antiquity of certain Subordinate Types of Fresh-water and Land Mollusca," occur two slight errors which may be misleading to future students pursuing the same line of research. On page 47 he lists seven species of fossil *Unionidæ* from the Laramie strata of Wyoming and Utah, together with a second series of seven recent Uniones "as their respective congeners." Congeneric with the *U. Couesi* White is written *Unio complanatus* Solander. Since the doctor gives a list of *Unionidæ* inhabiting the "*waters of the Mississippi basin*" as congeneric with the fossil forms, *U. complanatus* Sol. is out of place in such a list, being an inhabitant *solely* of the Atlantic slope, together with the numerous other species of which it is a type. This species is *not* found west of the Alleghanies. This fact is important in the matter of the geographical distribution of the recent *Unionidæ*. A slight error in the determination, or the nomenclature, of the species placed as congeneric with *U. Endlichi* White occurs. Doctor White evidently means *Unio gibbosus* Barnes. *Unio gibbus* was described by Spengler in "Skrivter af Naturhistorie Selskabet," vol. iii (1792,) and the habitat given is Tranquebar.

Dexter, Iowa, July 6, 1880.

10. *Packard's Zoology.*—A letter has been received from Professor Packard in reply to Professor Wilder's criticism of his work, as regards the accounts of the brains of vertebrates, on a former page of this volume. We cite from it a single sentence: "The book was not designed to give a monographic or even tolerably complete account of the brains of the Vertebrates; this would be out of place in such a work."

III. Miscellaneous Scientific Intelligence.

1. *The Seismological Society of Japan.*—A society under this name has been formed in Tokio, Japan, with the purpose of encouraging the study of earthquake and volcanic phenomena in that country. Its officers are I. Z. Hattori, President; Prof. John Milne, Vice-President, and Prof. W. S. Chaplin, Secretary. These names are already familiar to students of Seismology and are sufficient guarantee that the new society will be a valuable acquisition to the science. At a general meeting held April 26, 1880, a paper was read by Prof. Milne,* which he summarizes as follows: "What I have attempted has been to show the position which the study of earthquakes and volcanoes occupies in the scheme, waiting to be worked out, for the elucidation of the natural laws upon which all terrestrial things appear to be dependent. After this I give a condensed summary of the work which has been done in this country toward carrying out this scheme."

The paper, which is of considerable length, is printed in full in the account of the meeting in the Japan Gazette of May 1, 1880. One item of general interest is the actual horizontal movement of an earth particle during an earthquake shock, which Prof. Milne and his associates have succeeded in measuring instrumentally. The movement in seven cases, reported from his own and Mr. E. Knipping's observations, varied from 1·7 mm. to "4 or 5 mm.," with a mean of 2·9 mm. Prof. Milne described briefly several modifications of existing seismometers and at the close of his paper, Prof. J. A. Ewing exhibited a new form of seismograph. We quote the description of it somewhat condensed.

"The principle on which it is based is the well known one that the bob of a long pendulum may be assumed to be sensibly stationary during most shocks. Two levers at right angles to each other have their short arms kept in continuous contact with the very massive bob of a pendulum twenty feet long. The levers are joined to their supports so that each is affected only by the component of the movement resolved in its own direction. They are also unaffected by torsion of the pendulum or any other kind of relative motion of its parts. The long ends of the levers press gently against two smoked glass plates which are kept revolving continuously and uniformly by clockwork. So long as no earthquake occurs each lever traces one and the same circle over and over again on its revolving plate. The earthquake causes the

* Professor Milne has given some of his results in a letter to *Nature*, vol. xxii, p. 208, July 1, 1880.

lever to move across this line, and so records an undulating line on each plate. These lines enable the movements to be measured and their relation to the time, from which the amplitude, velocity and direction of the horizontal motion of a point on the earth's surface are determined at every instant throughout the whole disturbance. The pendulum is not suspended from the roof of the house but from a separate rigid frame; and the levers with their recording discs are attached firmly to a wooden post stuck in the ground and cut off a few inches above the surface."

The new society is well located for effective work, as earthquake shocks seem to occur in Japan *almost daily*. A letter from Prof. Milne (June 12) speaks of "*over fifty* during the last two months," one night five, and another night three. C. G. R.

2. *Science: A weekly Record of Scientific Progress;* JOHN MICHELS, editor. Vol. I, No. 1 and 2, New York, July 3 and 10, 1880. The aim of this new Journal is stated to be "to afford scientific workers in the United States the opportunity of promptly recording the fruits of their researches and facilities for communication between one another," etc. It aspires "to take the position which 'Nature' so ably occupies in England." The necessity of the existence of such a Journal may perhaps be questioned, in view of the prompt and wide distribution of "Nature" in this country, although it would certainly be of value if it could maintain the position suggested in the prospectus, which it certainly has not done thus far.

The author of the ingenious story about the "diaphote" will be amused to see a column in "Science" devoted to the subject, in which the remarkable invention of the supposed "Dr. H. E. Licks" (=*helix*) is described in full detail. The original account, from which this quotation was made, is so clearly worked out in all the minor points and so ingeniously plausible that it is, perhaps, not strange that so many have been deceived. It deserves to take a place beside the famous "moon hoax" of years ago.

3. *Science Record:* A Monthly Bulletin of Scientific Researches, Discoveries and Reports on the Pacific Coast. San Francisco, Cal. (Dewey & Co.). The "Science Record" was commenced in February, 1879. The number recently received (vol. ii, No. 3) contains an article on the Origin of Lakes, by Prof. John LeConte, the first of a series to be published in future numbers; it also contains an account of the monthly meeting of the California Academy of Sciences, an article on the U. S. Fish Commission on the Pacific Coast, and other interesting matter. This Journal promises to fill a valuable place as a medium of publication of scientific observation in the western part of the United States.

OBITUARY.

COUNT LOUIS FRANÇOIS DE POURTALES, the well-known pupil and companion of Agassiz, Keeper of the Museum of Comparative Zoology at Cambridge, Mass., died July 18, in the fifty-eighth year of his age. A notice will be given in a subsequent number of the Journal.

Professor EDUARD GRUBE, of Breslau, died June 23, 1880, at the age of sixty-eight years.

Chart showing the Eccentricity of the Earth's Orbit for 3,250,000 years before, and 1,260,000 years after A. D. 1850.

Ordinates are at Intervals of 50,000 years; full curve, Stockwell's; dotted curve, LeVerrier's.

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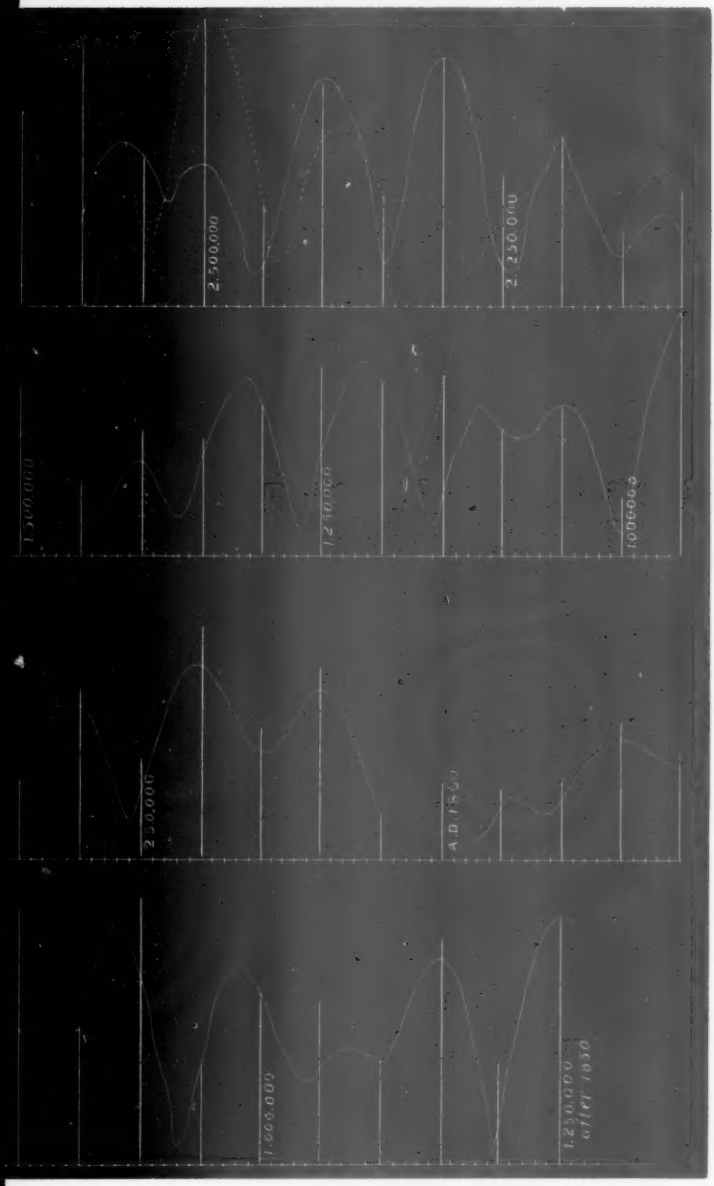

rve, Stockwell's; dotted curve, LeVerrier's.